

*Philip Earl Stanhope*







# PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

# A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

# I N G E N I O U S,

I N M A N Y

Confiderable Parts of the WORLD.

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VOL. XLIX. PART I. For the Year 1755.

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# PHILOSOPHICAL TRANSACTIONS.

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## The CONTENTS.

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PART I. VOL. XLIX.

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- I. *De Pressionibus Ponderum in Machinis motis.* Page 1.
- II. *An Investigation of a General Rule for the Resolution of Isoperimetrical Problems of all Orders.* By Mr. Thomas Simpson, F. R. S. p. 4.
- III. *Part of a Letter from John Huxham, M. D. F. R. S. to W. Warfon. F. R. S. in relation to the Effects of Lightning at Plymouth.* p. 16.
- IV. *A remarkable Case of a morbid Eye: By Mr. Edward Spry, Surgeon, at Plymouth, in Devonshire.* p. 18.
- V. *A Supplement to the Account of a distempered Skin, published in the 424th Number of the Philosophical Transactions.* By Mr. Henry Baker, F. R. S. p. 21.
- VI. *An Extract of the Substance of Three Letters from Isaac Jamineau, Esq; his Majesty's Consul at Naples, to Sir Francis Hoskins Eyles Stiles, Bart. and F. R. S. concerning the late Eruption of Mount Vesuvius.* p. 24.

# C O N T E N T S.

- Extract of another Letter from Mr. Jamineau, the British Consul at Naples, to Sir F. H. Eyles Stiles, dated Decem. 21, 1754.* p. 27.
- Extract of a Letter from the same Gentleman to the same, dated Decem. 28, 1754.* p. 28.
- VII. *An Account of the Species of Plant, from which the Agaric, used as a Styptic, is prepared. By Mr. William Watson, F. R. S.* p. 28.
- VIII. *An Account of a Mountain of Iron Ore, at Taberg in Sweden, in a Letter to Mr. Peter Collinson, F. R. S. By Peter Ascanius, M. D. Translated from the Latin by Mr. Emanuel Mendes de Costa, F. R. S.* p. 30.
- IX. *An Account of an extraordinary Case of a Child. By Mr. Richard Guy, Surgeon.* p. 34.
- X. *Extracts of Two Letters from Mr. James Lat-  
terman, Student in Physic and Surgery, to Dr.  
Schlosser, now residing in London, concerning the  
Effects of the Agaric of the Oak, after some of  
the most capital Operations in Surgery. To which  
are added, Some remarkable Experiments made  
upon the Arteries of Horses, with the Powder  
of the Lycoperdon, or Lupi Crepitus; by Mon-  
sieur La Fosse, Farrier to the King of France.  
Communicated by Mr. Joseph Warner, F. R. S.  
Surgeon of Guy's Hospital.* p. 36, 37.
- XI. *A Letter from James Parsons, M. D. F. R. S.  
to Thomas Birch, D. D. Secret. R. S. concerning  
the Use of Lycoperdon, in stopping Blood after  
Amputations.* p. 38.
- XII. *An Account of the State of the Thermometer,  
on the 8th and 9th of February 1755, in a Letter  
to John Canton, M. A. F. R. S. from Henry  
Miles, D. D. F. R. S.* p. 43.

# C O N T E N T S.

- XIII. *An Account of some Cases of Dropsies, cured by sweet Oil: In a Letter from William Oliver, M. D. F. R. S.* p. 46.
- XIV. *Observationes Eclipsium Satellitum Jovis habitæ Ulissipone in Regio Collegio Beatissimæ Virginis à Necessitatibus nuncupatæ, à Joanne Chevalier Cong. Oratorii Regalis & Societ. Lond. Socio, Anno 1754.* p. 48.
- XV. *An Account of those malignant Fevers, that raged at Rouen, at the End of the Year 1753, and the Beginning of 1754. By Mons. Le Car, M. D. Professor of Anatomy and Chirurgery at Rouen, F. R. S. Member of the Royal Academy of Sciences at Paris, and perpetual Secretary of that at Rouen.* p. 49.
- XVI. *An Account of the Death of Mr. George William Richman, Professor of experimental Philosophy, a Member of the Imperial Academy of Sciences at Petersburg. Translated from the High-Dutch.* p. 61.
- XVII. *An Account of a Roman Inscription found at Malton in Yorkshire, in the Year 1753. By John Ward, L L. D. Rhet. Prof. Gresh. and V. P. R. S.* p. 69.
- XVIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the worshipful Company of Apothecaries, for the Year 1754, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. Societat. Pharmaceut. Lond. Socius, Hort. Chelf. Præfectus & Prælector Botan.* p. 78.

# C O N T E N T S.

- XIX. *A Letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society, on the Advantage of taking the Mean of a Number of Observations, in practical Astronomy: By T. Simpson, F. R. S.* p. 82.
- XX. *An Account of the Success of Agaric, and the Fungus vinosus, in Amputations: By Mr. James Ford, Surgeon, of Bristol.* p. 93.
- XXI. *Queries sent to a Friend in Constantinople; by Dr. Mary, F. R. S.; and answered by his Excellency James Porter, Esq; his Majesty's Ambassador at Constantinople, and F. R. S.* p. 96.
- XXII. *Extracts of Two Letters to Thomas Hollis, Esq; concerning the late Discoveries at Herculaneum.* p. 109.
- XXIII. *Copy of a Letter from a learned Gentleman of Naples, dated February 25, 1755, concerning the Books and antient Writings dug out of the Ruins of an Edifice near the Site of the old City of Herculaneum; to Monsignor Cerati, of Pisa, F. R. S. sent to Mr. Baker, F. R. S. and by him communicated; with a Translation by John Locke, Esq; F. R. S.* p. 112.
- XXIV. *An Account of the several Earthquakes of late felt at Constantinople; by his Excellency James Porter, Esq; his Majesty's Ambassador at that Place; in a Letter to the Reverend Mr. Wetstein, F. R. S. dated February 15, 1755.* p. 115.
- XXV. *Letters of Henry Eeles, Esq; concerning the Cause of the Ascent of Vapour and Exhalation, and those of Winds; and of the general Phenomena*

## C O N T E N T S.

*mena of the Weather and Barometer; to the Rev. Thomas Birch, D. D. Secr. R. S.*

p. 124. 126. 151.

- XXVI. *Remarks upon a petrified Echinus of a singular kind, shewn to the Royal Society, April 24, 1755, by the Rev. Richard Pococke, LL. D. Archdeacon of Dublin, and F. R. S. found on Bunnan's-Land, in the Parish of Bovington in Hertfordshire, which is a Clay, and supposed to have been brought with the Chalk, dug out of a Pit in the Field. By James Parsons, M. D. and F. R. S.*

p. 155.

- XXVII. *Two Letters concerning Toxicodendron. Let. I. From the Abbé Mazeas, F. R. S. to the Rev. Stephen Hales, D. D. F. R. S. Translated from the French, by James Parsons, M. D. and F. R. S.*

p. 157.

*Let. II. From Mr. Philip Miller, F. R. S. to the Rev. Thomas Birch, D. D. Secret. R. S.*

p. 161.

- XXVIII. *A Letter to the Right Honourable the Earl of Macclesfield, President of the Royal Society, concerning the Method of constructing a Table for the Probabilities of Life at London, from the Reverend William Brakenridge, D. D. and F. R. S.*

p. 167.

- XXIX. *Some Account of a Sheep, shew'd alive to the Royal Society, in November 1754, having a monstrous Horn growing from his Throat; the stuffed Skin of which, with the Horn in situ, is now in the Museum of the Society. By James Parsons, M. D. F. R. S.*

p. 183.

- XXX. *A Dissertation upon the Cancer of the Eyelids, Nose, great Angle of the Eye, and its neighbour-*



# CONTENTS

*neighbouring Parts, commonly called the Noli-me-tangere, deemed hitherto incurable by both Antients and Moderns, but now shewn to be as curable as other Distempers. Addressed to the Royal Society of London by Mons. Daviel, consulting Surgeon in ordinary, and Oculist to the King; Master of Arts, and of Surgery at Mar-seilles; Royal Professor and Demonstrator of Anatomy of the same City; Member of the Academy of Sciences of Toulouse, Bologne, and that of Surgery of Paris; and translated from the French by James Parsons, M. D. and F. R. S.*

p. 186.

XXXI. *An Account of four Roman Inscriptions, cut upon three large Stones, found in a ploughed Field near Wroxeter in Shropshire, in the Year 1752: With some Observations upon them, by John Ward, LL.D. Rhet. Prof. Gresh. and V. P. R. S.*

p. 196.

XXXII. *Some Observations upon an American Wasps-Nest, shewn to the Royal Society: By Mr. Israel Mauduit, F. R. S.*

p. 205.

XXXIII. *An Extract of a Letter written by the Magistrates of the City of Mascali, in Sicily, and sent from their public Office to Naples, concerning a late Eruption of Mount Ætna. Translated from the Italian.*

p. 209.

XXXIV. *Some Account of the Charr-Fish, as found in North-Wales. In a Letter from the Rev. Mr. Farrington, of Dinas, near Caernarvon, to Mr. Thomas Collinson, of London. Communicated by Mr. Peter Collinson, F. R. S.*

p. 210.

XXXV.

# C O N T E N T S.

- XXXV. *A Method proposed to restore the Hearing, when injured from an Obstruction of the Tuba Eustachiana. By Mr. Jonathan Wathan, Surgeon, in Devon-Tire-Square.* p. 213.
- XXXVI. *Tentamen Chemicum de Calcis vivæ Actione in Salem volatilem alcaliâ. Auctore Johanne Alberto Schloffer, Ukra, celtine, M. D.* p. 222.
- XXXVII. *An Account of a very remarkable Case of a Boy, who, notwithstanding that a considerable Part of his Intestines were forced out by the Fall of a Cart upon him, and afterwards cut off, recovered, and continues well.* p. 238.
- XXXVIII. *An Account of some Experiments on the Sensibility and Irritability of the several Parts of Animals; in a Letter from Richard Brocklesby, M. D. F. R. S. to the Rev. Thomas Birch, D. D. Secr. R. S.* p. 240.
- XXXIX. *An Account of Worms in Animal Bodies. In a Letter to Thomas Birch, D. D. Secr. R. S. from Frank Nicholls, M. D. Med. Reg. and F. R. S.* p. 246.
- XL. *An Account of some remarkable Insects of the Polype kind, found in the Waters near Brussel's in Flanders. In a Letter to Tho. Birch, D. D. Secret. R. S. from T. Brady, M. D. Physician to his Highness's Prince Charles of Lorraine.* p. 248.
- XLI. *A short Account of some new Astronomical and Physical Observations made in Asia; and communicated to Matthew Maty, M. D. F. R. S. by his Excellency Mr. Porter, his Majesty's Ambassador at Constantinople, and F. R. S.* p. 251.

# CONTENTS.

- Extract of a French Letter, transmitted to his Excellency James Porter, Esq; by the English Consul at Aleppo.* p. 252.
- XLII. *Some Observations proving, that the Fetus is in part nourished by the Liquor Amnii.* By Malcolm Fleming, M. D. p. 254.
- XLIII. *An Account of the Success of Agaric in Amputations, &c. in a Letter from Mr. William Thornhill, late Surgeon to the Infirmary at Bristol, to Robert Dingley, Esq; F. R. S.* p. 264.
- XLIV. *Lunæ defectus Elbis à Doctore Joanne Mendefio Sachetto Barbosa, Philosophiæ & Medicinæ Professore, Regiæ Societatis Londinensis, Regalis Elbenis Nosocomii Medico, observatus Die 27--28 Martii, Anno 1755. Communicated by J. De Castro Sarmiento, M. D. F. R. S.* p. 265.
- XLV. *A Letter to George Lewis Scot, Esq; F. R. S. concerning the Number of People in England; from the Reverend William Brakenridge, D. D. Rector of St. Michael Bassishaw, London, and F. R. S.* p. 268.
- XLVI. *An Attempt to explain two Roman Inscriptions, cut upon two Altars, which were dug up some time since at Bath.* By John Ward, LL D. Rhet. Prof. Gresh. and V. P. R. S. p. 285.
- XLVII. *An Account of a remarkable Echinus: By Gustavus Brander, Esq; F. R. S.* p. 295
- XLVIII. *An Account of the Impression on a Stone dug up in the Island of Antigua, and the Quantity of Rain fallen there for Four Years: In a Letter from the Reverend Mr. Francis Byam to William Fauquier, Esq; F. R. S.* p. 295.
- XLIX.

## C O N T E N T S.

- XLIX. *A Letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society, concerning the Stones mentioned in the preceding Article* By Mr. Arthur Pond, F. R. S. p. 297.
- L. *An Account of the Effects of Lightning in the Danish Church, in Wellclose-Square.* By Gustavus Brander, Esq; F. R. S. p. 298.
- LI. *Electrical Experiments, made in pursuance of those by Mr. Canton, dated December 3, 1753; with Explanations by Mr. Benjamin Franklin. Communicated by Mr. Peter Collinson, F. R. S.* p. 300.
- LII. *Extract of a Letter concerning Electricity, from Mr. B. Franklin to Monsr. Delibard, inclosed in a Letter to Mr. Peter Collinson, F. R. S.* p. 305.
- LIII. *A Letter concerning the Effects of Lightning at Dorking in Surrey, from Mr. William Child to Mr. James Pitfold. Communicated by Mr. Peter Collinson, F. R. S.* p. 309.
- LIV. *An Account of the great Benefit of blowing Showers of fresh Air up through distilling Liquors.* By Stephen Hales, D. D. F. R. S. p. 312.
- LV. *An Account of the great Benefit of Ventilators in many Instances, in preserving the Health and Lives of People, in Slave and other Transport Ships.* By Stephen Hales, D. D. F. R. S. p. 332.
- LVI. *An Account of some Trials to cure the ill Taste of Milk, which is occasioned by the Food of Cows, either from Turnips, Cabbages, or autumnal Leaves, &c. Also to sweeten stinking Water, &c.* By Stephen Hales, D. D. F. R. S. p. 339.
- LVII.

# CONTENTS.

- LVII. *Extract of a Letter of Thomas Barker, Esq; to the Rev. James Bradley, D. D. Astronomer Royal, and F. R. S. concerning the Return of the Comet, expected in 1757, or 1758.* p. 347.
- LVIII. *An Account of an extraordinary and surprising Agitation of the Waters, though without any perceptible Motion of the Earth, observed in various Parts of this Island, both maritime and inland, on Novem. 1, 1755, chiefly about the Time that the more violent Commotions of both Earth and Waters so extensively affected many very distant Parts of the Globe: In several Letters transmitted to the Royal Society.*
- Letter I. *From Mr. John Robertson, F. R. S. to Tho. Birch, D. D. Secret. R. S. At Portsmouth in Hampshire.* p. 351.
- Let. II. *From Philip Carteret Webb, Esq; F. R. S. to Tho. Birch, D. D. Secr. R. S. In Suffex, and the Southern Parts of Surrey.* p. 353.
- Let. III. *From Swithin Adee, M. D. F. R. S. to Philip Carteret Webb, Esq; F. R. S.* p. 357.
- Let. IV. *From Swithin Adee, M. D. F. R. S. to Philip Carteret Webb, Esq; F. R. S.* ibid.
- Let. V. *From Mr. John Hodgson to Philip Carteret Webb, Esq; F. R. S.* p. 358.
- Extract of Letter VI. From Cranbrook in Kent, to William Tempest, Esq; F. R. S.* p. 360.
- Extract of Let. VII. Communicated by John Pringle, M. D. F. R. S.* p. 360.
- Let. VIII. *From Mr. Henry Mills to T. Birch, D. D. Secret. R. S.* p. 361.
- Let. IX. *An Account of the Agitation of the Water in Peerless-Pool, near Old-Street, London. By Tho. Birch, D. D. Secr. R. S.* p. 362.
- Let. X.

# C O N T E N T S.

- Let. X. *An Account of the Agitation of the Water at Rochford in Essex, Nov. 1, 1755, in a Letter from the Rev. Mr. Thomlinson to the Rev. Dr. Sykes, D. D. Dean of Burien. Communicated by Tho. Birch, D. D. Secret. R. S.* p. 364.
- Let. XI. *From Mr. Richard Philips. Communicated by Lewis Crusius, D. D. F. R. S. In Berkshire, near Reading.* p. 365.
- Let. XII. *An Account of the same. Communicated by the Rev. John Blair, LL. D. F. R. S.* p. 367.
- Let. XIII. *In Oxfordshire, at Shirburn-Castle, the Seat of the Earl of Macclesfield, Pres. R. S. as appears by the following Account, communicated by his Son the Lord Viscount Parker, F. R. S. there upon the Spot.* p. 368.
- Let. XIV. *From John Huxham, M. D. F. R. S. to Mr. William Watson, F. R. S. In Devonshire and Cornwall; at Plymouth, Mounts-Bay, Penzance, &c.* p. 371.
- Let. XV. *From the Rev. W. Borlase, A. M. F. R. S. to the Rev. Charles Lyttleton, LL. D. Dean of Exeter.* p. 373.
- Extract of Let. XVI. From Swanzey, dated Dec. 8, 1755. Communicated by the Rev. John Blair, LL. D. F. R. S.* p. 379.
- Extract of Let. XVII. From Mr. Wm. Arderon, F. R. S. to Henry Baker, F. R. S.* p. 380.
- Let. XVIII. *To Mr. Wm. Arderon, F. R. S. from Mr. Tho. Barber in Yarmouth, concerning the Earthquake on Novem. 1. Communicated by Mr. Arderon.* p. 380.
- Let. XIX. *From the Rev. Mr. John Harrison, Rector of Hawkeshead in Cumberland, to the Right Rev. Dr. Edm. Keene, Ld. Bishop of Chester.* p. 381.
- Extract*

# CONTENTS.

- Extract of Let. XX. From the Honourable and Rev. Dr. Spencer Cowper, Dean of Durham, to his Brother the Right Hon. William Earl Cowper, F. R. S.* p. 385.
- Let. XXI. To Dr. John Stevenson, Physician at Edinburgh. Communicated by Dr. John Pringle, F. R. S. Scotland, at Loch Ness.* p. 388.
- Extract of Let. XXII. From Sir James Colquhoun, of Luis, to Mr. Fletcher, Member of Parliament, dated Rosedoe, Dec. 8, 1755. Communicated by Dr. Pringle, F. R. S. At Loch Lommond.* p. 390.
- Let. XXIII. Communicated by Richard Brocklesby, M. D. F. R. S.* p. 393.
- Let. XXIV. An Account of an extraordinary Alteration in the Diths of Toplitz in Bohemia, on the first of November 1755, in a Letter from Father Joseph Steplin to James Short, M. A. F. R. S. Translated from the Latin.* p. 395.
- Extract of Letter XXVI. From Mr. De Hondt, of the Hague, concerning the Agitation of the Waters, Novem. 1. 1755. Translated from the French, and communicated by T. Birch, D. D. Secr. R. S.* p. 396.
- Extract of Let. XXVII. From Monsr. Allamond, Professor of Philosophy at Leyden, and F. R. S. dated there Decem. 9, 1755, to Mr. Trembley, F. R. S. Translated from the French.* p. 397.
- LIX. An Account of the Earthquake, Novem. 1, 1755, as felt in the Lead Mines in Derbyshire; in a Letter from the Rev. Mr. Bullock to Lewis Crusius, D. D. F. R. S. Letter I.* p. 398.
- Let. II. An Account of the Earthquake at Lisbon, Nov. 1, 1755, in Two Letters from Mr. Wol-tail, Surgeon, to J. Parsons, M. D. F. R. S.* p. 402.
- Letter III.* p. 408.
- Let. IV.*

## C O N T E N T S.

- Let. IV. *A Copy of Part of Two Letters, written by John Mendes Saccheri, M. D. F. R. S. to Dr. De Castro, F. R. S. dated from the Fields of Lisbon, on the 7th of November, and the 1st of December, 1755.*
- Extract of Let. V. From Mr. J. Latham, dated at Zfuiqueira, December 11, 1755, to his Uncle in London. Communicated by Peter Daval, Esq; Secret. R. S.* p. 411.
- Let. VI. *Observations, made at Co'ares, on the Earthquake at Lisbon, of the 1st of November 1755, by Mr. Stoqueler, Consul of Hamburgh. Communicated by Mr. H. Baker, F. R. S.* p. 413.
- Extract of Let. VIII. To Tilman Henkel, Esq; Merchant in London, concerning the Earthquake at Oporto in Portugal, Nov. 1, 1756. Communicated by Mr. John Ellicot, F. R. S.* p. 418.
- Let. IX. *Extract of Two Letters to Mr. Plummer, Merchant in London, from Oporto, concerning the Earthquake felt there. Communicated by Mr. John Ellicot, F. R. S.* p. 419, 421.
- Extract of Let. XI. From Oporto, by the Western, Capt. Knowler, dated Nov. 1, 1755. Communicated by Theodore Jacobson, Esq; F. R. S.* p. 421.
- Extract of Let. XII. From Monsf. Muysson to Monsf. Bazin, concerning the Earthquake at Oporto, Nov. 1, 1755. Communicated by M. Maty, M D. F. R. S. Translated from the French.* p. 422.
- Extract of Let. XIII. From Madrid, to the Spanish Consul, residing in London. Translated from the Spanish. Communicated by Mr. John Ellicot, F. R. S.* p. 423.
- Let. XIV. *An Account of the Earthquake at Cadiz, Nov. 1, 1755, in a Letter from Mr. Ben. Bewick, Merchant*



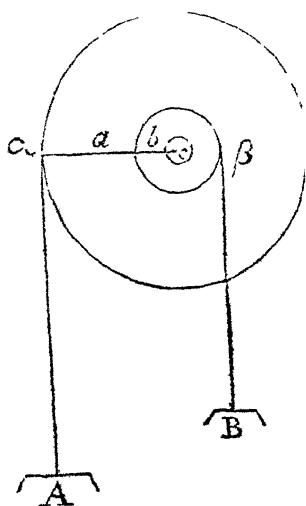
# CONTENTS.

- Merchant there, to Mr. Joseph Paice, Merchant in London.* p. 424.
- Lct. XV. *An Account of the Earthquake at Cadiz, in a Letter to the Spanish Ambassador at the Hague, from Don Ant. d' Ulloa, F. R. S.* p. 427.
- Lct. XVI. *An Account of the Earthquakes, that happened in Barbary, inclosed in a Letter from General Fowke, Governor of Gibraltar, to the Rt. Hon Henry Fox, Esq; one of his Majesty's principal Secretaries of State. Communicated by the Rt. Hon. Philip Ld. Visc. Royston, F. R. S.* p. 428.
- Lct. XVII. *An Account of the Earthquake in the Island of Madeira, Nov. 1, 1755, in a Letter from Dr. Tho. Heberden, to his Brother Dr. William Heberden, F. R. S.* p. 432.
- Lct. XVIII. *Another Account of the same Earthquake at Madeira, in a Letter from Mr. C. Chambers to his Father, in London.* p. 435.
- Lct. XIX. *An Account of the late Earthquakes of Nov. 1, and 9, 1755, as felt at Neufchâtel in Switzerland.* p. 436.
- Extract of* Lct. XX. *An Account of the Earthquake felt at Geneva, Nov. 9, 1755.* p. 438.
- Lct. XXI. *An Account of the Earthquake felt at Boston in New England, Nov. 18, 1755.* p. 439.
- Lct. XXI. *An Account of the Earthquake felt in New York, Nov. 18, 1755.* p. 443.
- Lct. XXII. *An Account of the Earthquake felt in Pennsylvania, Nov. 18, 1755.* p. 444.

# I. De Pressionibus Ponderum in Machinis motis.

Read at the Royal Society, June 27, 1754.

**A** Nimus erat aliquando in legem resistantiæ, quam patiuntur corpora in superficië aquæ mota, inquirendi. Suasit hoc cura, quam dudum mihi imposuit officii ratio, scientiarum navalium, quarum pleræque vel ipsi resistantiæ theoriæ innituntur, vel ita sunt cum eadem connexæ, ut resistantiam ipsam supponant cognitam. Ratiociniis et calculo interdum insuperabili fidere nolui, experimenta licet plurima pro omni casu non sufficere vidi; experimenta cum ratiociniis ea propter conjungere statui, modumque quæsi per experimenta in leges resistantiæ inquirendi.



VOL. 49.

Representavi mihi corpus specificè levius aquæ stagnanti ad certam immersum profunditatem, idemque filo duas ambiente trocleas potentiæ ita junctum, ut, hâc suo præpondio verticaliter descendente, motu illud horizontali donaretur, quæsi ex tempore spatio et ponderibus datis, resistantiam pro quovis corpore determinare.

Non licuit heic, ut moris est, abstrahere à frictionibus,

tionibus, à rigiditate fili, ab inertia materiæ: introducenda erant hæc omnia in expressiōne vis acceleratricis, si ducta illa in elementum temporis monstraret verum celeritatis incrementum.

At nemo erat, quantum constitit, qui ita dilucidavit theoriā frictionis ex incrementō pressiōnis in machinis motis oriundæ, ut nexus cum primis mechanicæ principiis dilucide pateret; quod me invitavit ad indagandam solutionem, quam non ut omni numero perfectam, sed potius ab eruditis et meliora edoctis corrigendam et ulterius perficiendam, Illustrissimæ Societati proponam.

Repræsentat adjecta figura axem in peritrochio. Sit potentia movens  $A$ . distantia ipsius à centro motus  $a$ . Sit quoque pondus  $B$ , ejusque à centro distantia  $b$ . Sit radius axis, in quem frictio cadit  $= c$ . Pondus machinæ  $= M$  distantia centri virium a centro gravitatis  $= d$ . Quæritur jam pressio in axem, cum potentia descendens  $A$  machinam agitet.

Si jam pressio oriunda ex descendente potentia  $A$ , seu illa qua filum tenditur ad latus  $\alpha$ , appelletur  $\pi$ , erit ob actionis et reactionis æqualitatem pressio seu tensio ad alterum latus  $\beta = \frac{\pi a}{b}$ , unde integra pressio, excluso pondere machinæ funisque,  $= \pi + \frac{\pi a}{b} = \left(1 + \frac{a}{b}\right) \pi$ . Sit jam constans ratio pressiōnis ad frictionem ut  $1 : \mu$ ; erit frictio  $= \left(1 + \frac{a}{b}\right) \pi \mu$ ; et momentum hujus frictionis  $= \left(1 + \frac{a}{b}\right) c \pi \mu$ : momentum vero frictionis ex pondere machinæ  $= M c \mu$ ; quod priori momento adjectum dat  $\left(\left(1 + \frac{a}{b}\right) \pi + M\right) c \mu$ ; unde momentum potentiæ  

moventis

moventis =  $Aa - Bb - \left( \left( 1 + \frac{a}{b} \right) \pi + M \right) c \mu$ .

Cum vero momentum inertiae fit  $Aa^2 + Bb^2 + Md^2$ ,

erit vis acceleratrix =  $Aa - Bb - \left( \left( 1 + \frac{a}{b} \right) \pi + M \right) c \mu$ .

et pro acceleratione puncti  $\alpha$ , seu potentiae moventis  $A$ , habetur per principia mechanicae:

$$\left( \frac{Aa^2 - Bb^2 - \left( \left( 1 + \frac{a}{b} \right) \pi + M \right) ac\mu}{Aa^2 + Bb^2 + Md^2} \right) dt =$$

$dc$ ; ubi  $dt$  significat elementum temporis;  $dc$  vero incrementum velocitatis. Si autem  $A$  liberè cecidisset, fuisset  $\frac{A}{A} dt = dt'$ . Cum autem incrementa

vel decrementa velocitatum eadem temporis particula in eodem corpore genita sint ut vires generantes, licebit inferre ut  $dt' : dt' - dc = A$ : ad vim generantem decrementum celeritatis  $dt' - dc$ , quæ eadem vis est, quæ lapsum corporis retardat, filum tendit, et ad latus  $\alpha$  premit; unde substitutis valoribus habetur analogia sequens

$$1 : 1 - \frac{Aa^2 - Bb^2 - \left( \left( 1 + \frac{a}{b} \right) \pi + M \right) ac\mu}{Aa^2 + Bb^2 + Md^2} = A : \pi$$

ideoque  $\pi =$

$$\frac{ABb^2 + AMd^2 + ABab + \left( \left( 1 + \frac{a}{b} \right) \pi + M \right) Aac\mu}{Aa^2 + Bb^2 + Md^2}$$

ex qua æquatione invenitur  $\pi =$

$$\frac{ABb^2 + AMd^2 + ABab + AMac\mu}{Aa^2 + Bb^2 + Md^2 - \left( 1 + \frac{a}{b} \right) Aac\mu}$$

$$\text{et} \dots \frac{\pi a}{b} = \frac{ABab + AMd^2 \frac{a}{b} + ABa^2 + AM \frac{a^2 c \mu}{b}}{Aa^2 + Bb^2 + Md^2 - \left(1 + \frac{a}{b}\right) Aac\mu}$$

$$\text{et pressio integra} = \pi + \frac{\pi a}{b} =$$

$$\frac{AB(a+b)^2 + AM(a^2 + ac\mu) \left(1 + \frac{a}{b}\right)}{Aa^2 + Bb^2 + Md^2 - \left(1 + \frac{a}{b}\right) Aac\mu}$$

Si jam frictionem et pondus machinæ excludere placuerit, habetur pressio integra =  $\frac{AB(a+b)^2}{Aa^2 + Bb^2}$ : et si,

ut in troclea evenit, supponatur  $a = b$ , erit pressio integra =  $\frac{AB(a+a)^2}{(A+B)a^2} = \frac{4AB}{A+B}$

Christianus Hée,

*Professor Maibef. et Phys. Experim. in Statu Navali Hafniensi, Societatum Scient. Hafniensis et Berolinensis Membrum.*

## II. *An Investigation of a General Rule for the Resolution of Isoperimetrical Problems of all Orders. By Mr. Thomas Simpson, F. R. S.*

Read Jan. 9, 1755. **T**HE different species of problems comprehended under the name of Isoperimetrical ones, are of much greater extent than the name imports; since, not only the determination of the greatest areas and solids, under equal

equal perimeters or bounds (whence the name is derived), but whatever relates to the Maxima and Minima of quantities depending on a line, space, or body, whereof the figure is unknown, is, by mathematicians, included under that denomination.

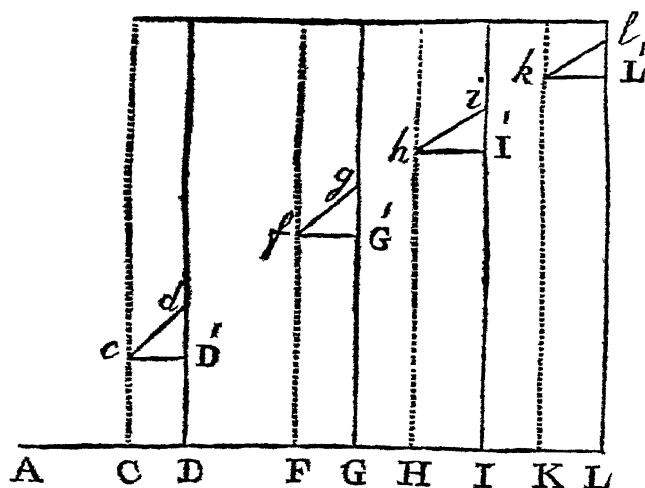
But notwithstanding the usefulness and great extent of this subject, nothing (that I know of) had been done thereon farther than the resolution of certain particular cases (such as finding the line of the swiftest descent, and the solid of the least resistance), 'till the celebrated mathematician M<sup>r</sup> Laurin, in his treatise of fluxions, gave the investigation of an elegant and very easy method, whereby the principal problems belonging to the first order may be resolved.

The paper I have now the honour to lay before the Society contains farther improvements on this subject: as it is by far more general than any thing yet offered, and is drawn up with a view to obviate the difficulties attending the resolution of a very intricate kind of problems, and thereby to open an easy way to some very interesting inquiries in natural philosophy, I cannot doubt of its meeting with a favourable reception.

*Lemma I.*

*Fig. 1.* At any given points  $D, G, I, L$ , in a right-line  $AL$ , suppose perpendiculars to be erected; and from any other given points  $c, f, b, k$ , at equal distances. ( $c D', f G', b I', k L'$ .) from the said perpendiculars, respectively, conceive right-lines  $c d, f g, b i, k l$ , to be drawn, to terminate somewhere in the said perpendiculars; let  $Q, R, S, T$ , denote

denote any quantities expressed in terms of  $AC$ ,  $cD'$ , and  $D'd$ , (independent of  $Cc$ ) and  $\mathcal{Q}$ ,  $R'$ ,  $S'$ ,  $T'$ , as many other quantities affected in the very same manner with  $A F$ ,  $f G'$ , and  $G'g$ ; and let  $\mathcal{Q}''$ ,  $R''$ ,  $S''$ ,  $T''$ , and  $\mathcal{Q}'''$ ,  $R'''$ ,  $S'''$ ,  $T'''$ , be quantities, still, expressed in the same manner, in terms of  $AH$ ,  $h I$ ,  $I i$ , and  $A K$ ,  $k L'$ ,  $L' l$ , respectively: 'tis proposed to find an equation expressing the relation of the indeterminate perpendiculars  $D'd$ ,  $G'g$ ,  $I i$ ,  $L' l$ , so that the quantity  $\mathcal{Q} + \mathcal{Q}'' - \mathcal{Q}' - \mathcal{Q}'''$  may be a Maximum or Minimum, at the same time that the values of the other quantities  $R + R' + R'' + R'''$ ,  $S + S' + S'' + S'''$ , and  $T + T' + T'' + T'''$ , are given, or continue invariable.



Put  $D'd = \alpha$ ,  $G'g = \beta$ ,  $I i = \gamma$ ,  $L' l = \delta$ ; and let the fluxion of  $\mathcal{Q}$  (supposing  $\alpha$  variable) be denoted by  $q\dot{\alpha}$ , that of  $R$ , by  $r\dot{\alpha}$ , &c. &c. then, since (by the nature of the proposition) the fluxion of  $\mathcal{Q} +$

$\mathcal{Q} + \mathcal{Q}' + \mathcal{Q}'' + \mathcal{Q}'''$ , as well as those of  $R + R' + R'' + R'''$ ,  $S + S' + S'' + S'''$ , &c. must be equal to nothing, we therefore

$$\text{have } \begin{cases} q\dot{\alpha} + q'\dot{\beta} + q''\dot{\gamma} + q'''\dot{\delta} = 0 \\ r\dot{\alpha} + r'\dot{\beta} + r''\dot{\gamma} + r'''\dot{\delta} = 0 \\ s\dot{\alpha} + s'\dot{\beta} + s''\dot{\gamma} + s'''\dot{\delta} = 0 \\ t\dot{\alpha} + t'\dot{\beta} + t''\dot{\gamma} + t'''\dot{\delta} = 0 \end{cases}$$

In order, now, to exterminate the fluxions  $\dot{\alpha}$ ,  $\dot{\beta}$ ,  $\dot{\gamma}$ ,  $\dot{\delta}$ , let these equations be respectively multiplied by 1,  $e$ ,  $f$ ,  $g$ , (yet unknown), and let all the products thence arising be added together, whence will be had

$$\overbrace{q + er + fs + gt \times \dot{\alpha} + q' + er' + fs' + gt' \times \dot{\beta} + q'' + er'' + fs'' + gt'' \times \dot{\gamma} + q''' + er''' + fs''' + gt''' \times \dot{\delta}} = 0.$$

$$\text{Make, now, } q' + er' + fs' + gt' = 0$$

$$q'' + er'' + fs'' + gt'' = 0$$

$$q''' + er''' + fs''' + gt''' = 0$$

From whence (there being as many equations as quantities,  $e$ ,  $f$ ,  $g$ , to be determined), the values of these quantities will be always given in terms of  $q'$ ,  $r'$ ,  $s'$ , &c. that is,  $e$ ,  $f$ ,  $g$ , will always be represented by quantities depending on  $q'$ ,  $r'$ ,  $s'$ , &c. (or on  $AF$ ,  $G'g$ , &c.) exclusive of  $q$ ,  $r$ ,  $s$ ,  $t$ , (or of  $AC$  and  $Dd$ ), which have nothing to do in these last equations.

But, because all the terms of the equation  $\overbrace{q + er + fs + gt \times \dot{\alpha} + q' + er' + fs' + gt' \times \dot{\beta} + q'' + er'' + fs'' + gt'' \times \dot{\gamma} + q''' + er''' + fs''' + gt''' \times \dot{\delta}} = 0$ , after the first ( $q + er + fs + gt \times \dot{\alpha}$ ) do vanish (by their coefficients being made equal to nothing), it is evident that  $q + er + fs + gt$  must also be  $= 0$ : which is an equation expressing the general relation of  $AC$ ,  $cD'$ , and  $D'd$ , with regard to

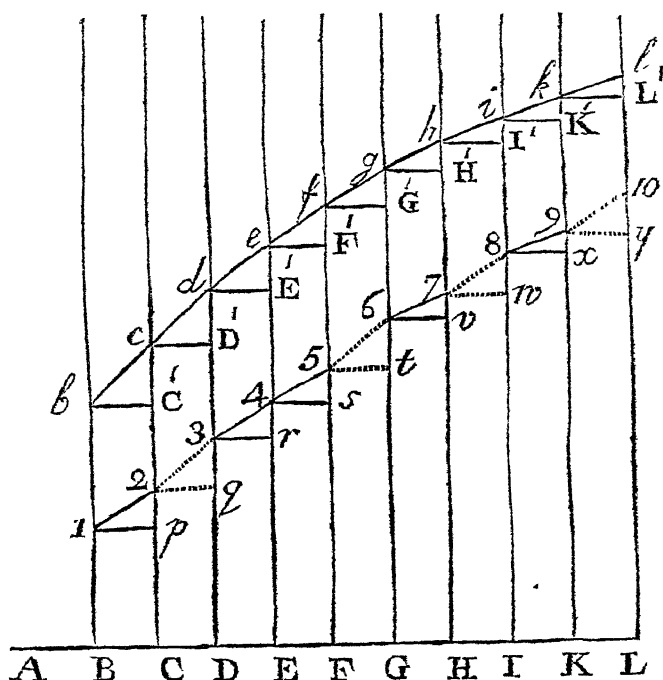


to the other proposed quantities  $AF, fG', G'g, \&c.$  whereon the coefficients  $e, f, g$ , depend: and this relation will, evidently, continue the same, at whatever distances from the line  $AL$ , the points  $c, f, h, k$ , are taken, as these distances have nothing to do in the consideration, all the proposed quantities (as well the  $Q$ 's as  $R$ 's,  $\&c.$ ) being (by hypothesis) expressed in terms intirely independent thereof.

*Lemma II.*

*Fig. 2.* Upon a given right-line  $BL$ , suppose perpendiculars  $Bb, Cc, Dd, \&c.$  to be erected at equal distances; and upon the same line  $BL$ , as a base, suppose a polygon  $Bb c d e f g h i k l L$  to be constituted, having its angular points  $b, c, d, \&c.$  posited in the said perpendiculars; let  $y$  denote the distance of any of these perpendiculars ( $Cc, Dd, \&c.$ ) from any given point  $A$ , in  $LB$  produced; and, supposing  $bC', cD', dE', \&c.$  to be drawn parallel to  $AB$ , let the base of any of the little triangles  $bC'c, cD'd, \&c.$  be represented by  $y'$ , and the perpendicular corresponding by  $x$  ( $y'$  being given, or the same, in every triangle, and  $x$  indeterminate): then, supposing  $Q, R, S, T$ , to denote any quantities expressed in terms of  $y, y'$ , and  $x$ , it is proposed to find an equation exhibiting the general relation of the quantities  $y, y'$ , and  $x$ , so that the sum of all the  $y'Q$ 's (resulting from the several triangles) may be a Maximum or Minimum, at the same time that the sums of all the  $y'R$ 's,  $y'S$ 's,  $\&c.$  are given quantities.

Because



Because the values of the quantities  $j'Q$ ,  $j'R$ ,  $j'S$ ,  $j'T$ , depending on the different triangles  $bC'c$ ,  $cD'd$ ,  $\&c.$  are supposed to be no-ways affected by the distances ( $Bb$ ,  $Cc$ ,  $\&c.$ ) of the bases of those triangles, from the base  $BL$  of the polygon, it is evident, that those values may be changed, by altering the species of one, or more, of the said triangles at pleasure, without any-ways affecting the values depending on the other triangles: for another polygon  $LB12345$ ,  $\&c.$  may be so described as to have all its sides, respectively, parallel to those of the former, excepting only those ( $23$ ,  $56$ ,  $78$ ,  $910$ ) you would have to be different: so that the whole variation in the

several sums (whether of the  $y'Q$ 's,  $y'R$ 's, or  $y'S$ 's, &c.) will depend intirely upon the difference of the particular triangles  $2q3$ ,  $cD'd$ ;  $5t6$ ,  $fG'g$ , &c. assigned.

Since, therefore, the values of the  $y'Q$ 's,  $y'R$ 's,  $y'S$ 's, &c. may be varied, at pleasure, by altering the species of any number of corresponding triangles ( $2q3$ ,  $cD'd$ ;  $5t6$ ,  $fG'g$ ;  $7w8$ ,  $bI'i$ ;  $9y10$ ,  $kL'l$ ), while the other triangles, and the values depending on them, remain the same, it is manifest, that, when the sum of the  $y'Q$ 's, answering to all the triangles, is a Maximum or Minimum, the sum of any number of them, taken at pleasure (other things remaining the same), will likewise be a Maximum or Minimum; and, consequently, that the sum of as many  $Q$ 's will, at the same time, be a Maximum or Minimum, because  $y'$  is every-where the same, or a constant quantity.

Hence, if the construction of the preceding Lemma be retained (supposing all the  $Q$ 's,  $R$ 's,  $S$ 's, &c. to be here expressed as before, in terms of  $AC$ ,  $cD'$ , and  $D'd$ , &c.) it is plain that the sum of all the  $Q$ 's, (or of the  $y'Q$ 's), depending on the said particular triangles (and consequently of all the  $y'Q$ 's in general), will be a Maximum or Minimum, when the general relation of  $y$ ,  $y'$ ,  $x'$ , (or of  $AC$ ,  $cD'$ ,  $D'd$ ), is expressed by the same equation  $q + er + fs + gt = 0$ , there determined: in which  $q$ ,  $r$ ,  $s$ ,  $t$ , represent the fluxions of  $Q$ ,  $R$ ,  $S$ ,  $T$ , divided by that of  $x'$  ( $= \alpha = D'd$ ), and wherein the coefficients  $e$ ,  $f$ ,  $g$ , will be constant quantities; because it is proved that their values depend intirely on the triangles  $fG'g$ ,  $bI'i$ ,  $kL'l$ , which remain the same,

same, let the perpendicular (or ordinate)  $Cc$  be taken at what distance you will from the given point  $A$ ; that is, let  $y$  stand for which you will of the distances  $AB, AC, AD, \&c.$   $\mathcal{Q} E. I.$

*Corollary.*

If the sides of the polygon  $b c d e f g h, \&c.$  be diminished, and their number increased *in infinitum*, the sum of all the  $j' \mathcal{Q}$ 's will (it is well known) be expressed by the fluent of  $j \mathcal{Q}$ ; the sum of all the  $j' R$ 's, by the fluent of  $j R, \&c.$  whence it follows, that, to have the fluent of  $j \mathcal{Q}$  (answering to a given value of  $y$ ) a Maximum, or Minimum, and the fluents of  $j R, j S, \&c.$  at the same time, given quantities, the relation of  $y, j$ , and  $\dot{x}$ , must be defined by the equation  $q + er + fs + g\epsilon = 0$ , above exhibited;  $q, r, s, \&c.$  being the respective fluxions of  $\mathcal{Q}, R, S, \&c.$  divided by that of  $\dot{x}$  (or  $\dot{x}'$ ); this quantity  $\dot{x}$ , or  $\dot{x}'$ , (in finding the said fluxions) being, alone, considered as variable. Hence we have the following

GENERAL RULE.

For the resolution of Isoperimetrical Problems, of all orders, take the fluxions of all the given expressions (as well that respecting the Maximum, or Minimum, as of the others whose fluents are to be given quantities), making that quantity ( $\dot{x}$ ) alone variable, whose fluent ( $x$ ) enters not into the said expressions; and, having divided everywhere by the second fluxion ( $\ddot{x}$ ), let the quantities hence arising, joined to general coefficients,

1,  $e, f, g, &c.$  (whose values will depend on the values given, and may be either positive or negative), be united into one sum, and the whole be made equal to nothing; from which equation the true relation of  $\dot{x}$  and  $\dot{y}$ , and of  $x$  and  $y$ , will be given, let the number of restrictions be what it will.

For an example of the general Rule here laid down, let the fluxions given be  $\frac{y \dot{x}^3}{\dot{y} y}$ , and  $\dot{x}$ ; the fluent of the former, corresponding to any given value of  $y$ , being to be a Minimum, and that of the latter, at the same time, equal to a given quantity. Here, taking the fluxions of both expressions (making  $\dot{x}$ , alone, variable), and dividing by  $\dot{x}$ , the quantities resulting will be  $\frac{3y \dot{x} \dot{x}}{\dot{y} y}$  and 1; so that, in this case, we have  $\frac{3y \dot{x} \dot{x}}{y y} + e = 0$ , and therefore  $\dot{x} = a^{\frac{1}{2}} y^{-\frac{1}{2}} \dot{y}$  (supposing  $a = -\frac{1}{3}e$ ). From whence, by taking the fluents,  $x = 2 a^{\frac{1}{2}} y^{\frac{1}{2}}$ , or  $x^2 = 4 a y$ , an equation answering to the common parabola.

If the absciffe of a curve be denoted by  $x$ , and the ordinate by  $y$ , it is known, that the several fluxions of the absciffe, curve-line, area, superficies of the generated solid, and of the solid itself, will be represented by  $\dot{x} \sqrt{x\dot{x} + y\dot{y}}$ ,  $y \dot{x}$ ,  $2 p y \sqrt{x\dot{x} + y\dot{y}}$ , and  $p y^2 \dot{x}$  respectively: if, therefore, the fluxions of these different expressions be taken as before (making  $\dot{x}$ , alone,

alone, variable), we shall get  $1 + \frac{e \dot{x}}{\sqrt{\dot{x}\dot{x} + y\dot{y}}} + f\dot{y} + \frac{g y \dot{x}}{\sqrt{\dot{x}\dot{x} + y\dot{y}}} + b y^2 = 0$ , being a general equation for determining the relation of  $x$  and  $y$ , when any one of the said five quantities (*viz.* abscisse, curve-line, area, superficies, or solid) is a Maximum or Minimum, and all, or any number of the others, at the same time, equal to given quantities; wherein the coefficients  $e, f, g$ , and  $b$ , may be either positive or negative, or nothing, as the case proposed may require. Thus, for example, if the length of the curve, only, be given, and the area corresponding is required to be a Maximum, our equation will then become  $\frac{e \dot{x}}{\sqrt{\dot{x}\dot{x} + y\dot{y}}} + f\dot{y} = 0$ , or  $a^2 \dot{x}^2 = y^2 \times \sqrt{\dot{x}\dot{x} + y\dot{y}}$  (by making  $a = -\frac{e}{f}$ ); whence  $\dot{x} = \frac{y \dot{y}}{\sqrt{a a - y y}}$ , and consequently  $x = a - \sqrt{a a - y y}$ , or  $2 a x - x^2 = y^2$ , answering to a circle; which figure, therefore, of all others, contains the greatest area, under equal bounds.

If together with the ordinate (which, here, is always supposed given) the abscissa, at the end of the fluent, be given likewise, and the superficies generated by the revolution of the curve about its axis be a Minimum; then, from the same equation, we have

$$1 + \frac{g y \dot{x}}{\sqrt{\dot{x}\dot{x} + y\dot{y}}} = 0: \text{ whence } \left( \text{making } a = -\frac{1}{g} \right)$$

$\dot{x}$  is

$\dot{x}$  is found  $= \frac{a \dot{y}}{\sqrt{yy - aa}}$ ; and, from thence,  $x =$   
 $a \times \text{hyp. log. } \frac{y + \sqrt{yy - aa}}{a}$ ; which equation, be-

ing impossible when  $y$  is less than  $a$ , shews that the curve (which is here the Catenaria) cannot possibly meet the axis about which the solid is generated; and, consequently, that the case will not admit of any Minimum, unless the first, or least given value of  $y$  exceeds a certain assignable magnitude.

When any, or all of the above-specified quantities are given, and the contemporary fluent of some other expression, as  $\overline{\dot{x}\dot{x} + \dot{y}\dot{y}}^m \times y \times \dot{y}$ , is required to be a Maximum, or Minimum; the equation (by taking the fluxion of this last expression, and joining it to the former) will then be  $\overline{\dot{x}\dot{x} + \dot{y}\dot{y}}^{1-2n} \times 2n \dot{x}y \dot{y}$

$$+ d + \frac{e \dot{x}}{\sqrt{\dot{x}\dot{x} + \dot{y}\dot{y}}} + f y + \frac{g y \dot{x}}{\sqrt{\dot{x}\dot{x} + \dot{y}\dot{y}}} + h y^2 = 0;$$

which, when  $m = 1$ , and  $n = -1$ , will be that defining the solid of the least resistance; and this, when the axis only is supposed to be given (without farther restrictions) will be expressed by

$$\overline{\dot{x}\dot{x} + \dot{y}\dot{y}}^{-2} \times -2 \dot{x}y \dot{y}^3 + d = 0, \text{ or } 2 y \dot{y}^3 \dot{x} =$$

$d \times \overline{\dot{x}\dot{x} + \dot{y}\dot{y}}$ ; being the case, first, considered by Sir Isaac Newton.— If both the length and the solid content be given, the equation will be

$$-2 \dot{x}y \dot{y}^3 \times \overline{\dot{x}\dot{x} + \dot{y}\dot{y}} + d + h y^2 = 0; \text{ but if,}$$

besides

besides these, the superficies is given likewise, it will then be  $-2 \dot{x} y \dot{y}^3 \times \overline{\dot{x} \dot{x} + \dot{y} \dot{y}}^{-2} + d + \frac{e y \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} + b y^2 = 0$ .

Thus, in like manner, by assuming  $m = -\frac{1}{2}$ , and

$n = \frac{1}{2}$ , we have  $\frac{y^{-\frac{1}{2}} \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} + d + \frac{e \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} +$

$f y + \frac{g y \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} + b y^2 = 0$ , for the general equation of the curve of the swiftest descent: which,

when  $e, f, g$ , and  $b$ , are, all of them taken equal to

nothing, will become  $\frac{y^{-\frac{1}{2}} \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} + d$ ; which is the

case, considered by so many Others, answering to the cycloid. When the length of the arch described in the whole descent (as well as the values of  $x$  and  $y$ )

is given, the equation will then be  $\frac{y^{-\frac{1}{2}} \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} + d +$

$\frac{e \dot{x}}{\sqrt{\dot{x} \dot{x} + \dot{y} \dot{y}}} = 0$ , or  $e - y^{-\frac{1}{2}} \dot{x}^2 = d^2$

$\times \overline{\dot{x} \dot{x} + \dot{y} \dot{y}}$ . And thus may the relation of  $x$  and  $y$  be determined, in other cases, and that under any number of restrictions.

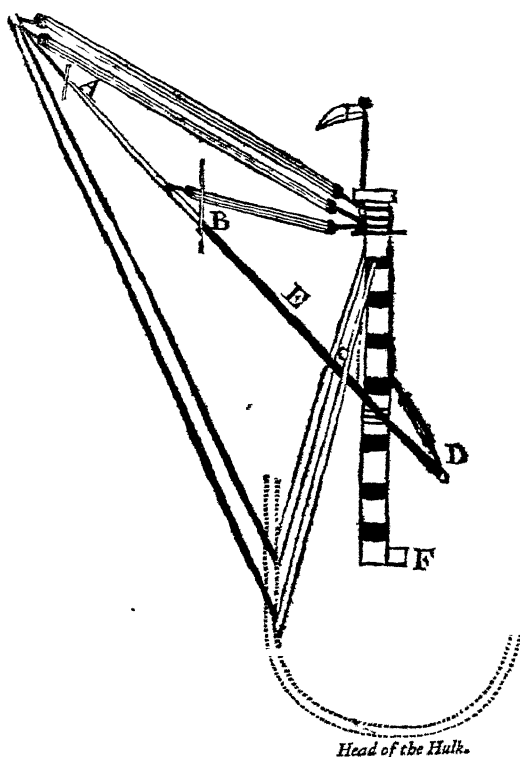


III. *Part of a Letter from John Huxham,  
M. D. F. R. S. to W. Watfon, F. R. S.  
in relation to the Effects of Lightning at  
Plymouth.*

Read Jan. 9, 1759. **S**unday, December 15, 1754, twenty-five minutes after one P. M. a vast body of lightning fell on the great hulk at Plymouth-dock, which serves to hoist in and fix the masts of the men of war. You may have some idea of it by this scheme, which was sent me by the lieutenant of the Peregrine, which lay close by the hulk, who saw it. It burst out about a mile or two to the westward of the hulk, and rushed with incredible velocity towards it. The piece of the Derrick cut out was at least eighteen inches diameter, and about fifteen or sixteen feet long: this particular piece was in three or four places begirt with iron hoops about two inches broad, and half an inch thick, which were completely cut in two by the lightning, as if done by the nicest hand and instrument. Two days after the accident I went on board myself, and examined the matter, and have added what I further observed.

The lightning was immediately succeeded by a dreadful peal of thunder, and that forthwith by the most violent shower of hail I ever saw in England, which fell only in and about this town, for a mile or two: there was very little of it at the dock, though only two miles distant. The hail-stones were as big as small nutmegs, much of the shape of some sort of beads cut into squares, a kind of a dodecahedra, quite pellucid as the clearest ice, with only a white speck in the middle, about the bigness of a pea. But that, for which I chiefly mention the whole, is,  
that

that they were all very nearly of the same bigness and figure. I examined thousands of them, as well as many other persons, who all found this similarity in size and form: they measured, immediately after they fell, near two inches round. I think this great uniformity in the shape of the hail must be owing to some saline principle, that determined the configuration. The uniform stellate form of snow is very remarkable, and seems owing to the nitro-aerious salt. Every one knows salts shoot into particular forms: and when the salts are compounded, the figure participates of both, as is seen in what is called the quadrangular nitre.



December 15, half past 1, P. M. wind S. W.

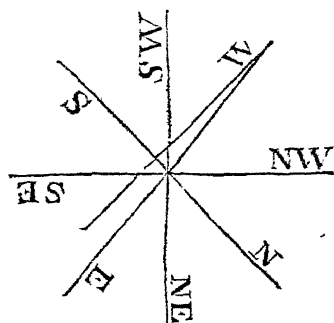
*A* to *B*, Cut all away by lightning.

*C*, The mast sprung by ditto, into the heart.

*D*, The lashing, forced by the shock off the Derrick's end, tho' the lashing round the mast was found and fast.

*E*, The Derrick, (or Direct), out of which a piece from *A* to *B* was cut off by the lightning: round this piece were three or four iron hoops, as described, which were cut asunder. The Derrick was at least eighteen inches diameter.

*F*, The chain-pump, split quite thro' by the lightning.



I do not know what the gentleman means by this. The wind was W. S. W. the lightning came from due west.

IV. *A remarkable Case of a morbid Eye :  
By Mr. Edward Spry, Surgeon, at Ply-  
mouth, in Devonshire.*

June 1751.

Read Jan. 9.  
1755.

**M**ARY the wife of Thomas Smal-  
dridge, a mariner of Plymouth,  
complained to me of a violent pain in her left eye,  
and sometimes of very acute pains in the temple of  
the same side, with some defect in her sight. She also  
imagined that her eye was bigger than ordinary; but,  
upon inspection, it appeared no bigger than the other.

The

The Cornea however became less transparent, and the pupil greatly dilated: but though the pain of her eye was so great, yet the blood-vessels of the conjunctive were no way enlarged, nor in the least redder than that and the sclerotic were before; and, from its whiteness, it appeared no more morbid than the other.

As to the cure: After having bled her plentifully in the arm, I applied a blistering-plaister to her neck, and a gentle repellent collyrium to wash her eye often; purging her every third or fourth day, as her strength permitted, with a bolus of about eight grains of calomel, given night and morning, with a solutive draught, pursuing this method for a month or six weeks; in which time she was bled three times more; and cautiously observed my directions in her diet.

But as this method proved of no effect, she consulted Doctor Martyne, an eminent physician of this town, who gave all the assistance he could, but with as little success, her pain increasing rather than diminishing; and as the pupil was now enlarged, and the cornea became more opaque, with great inflammation of the conjunctive and sclerotic, and an apparent prominence of the whole eye, when every thing hitherto failed, I tried two or three drastic purges; but these disagreeing very much, I was forced to return to my former method. I then cut a seton in her neck, which run very much; but all to no purpose, and she became still more miserable. The conjunctive became greatly inflamed, with an eversion of the upper lid, attended with great pain. I often made incisions with my lancet upon this coat, which bled plentifully, and gave her ease for a day

or two, and even took eight ounces of blood from the temporal artery. But the eye being greatly enlarged, and of so terrible an appearance, after all our endeavours for eight or ten months, I judged her disease to be a Carcinoma, and therefore proposed cutting out the whole eye as the only remedy.

Several skilful surgeons were consulted, and it was agreed to defer the operation, and trust to nature; though she was in that miserable condition; but at length her eye becoming much greater, and her pain being increased, I resolved upon the operation, lest the bones of the orbit might become carious.

Thus, having called in that eminent and famous physician Doctor Huxham, with some of the most skilful surgeons of this place, I performed the operation in the following manner; *viz.* The tumor was so very large, and the upper lid so distended, that I was obliged first to divide the orbicular muscle at the inner Canthus; and there began my incision round the upper part of the tumor; for the more convenient use of my knife. I had not cut deep when a great quantity of pus, like lymph, flow'd out upon me, with great force, like a fountain, and the tumor subsided a good deal: but pursuing the operation, I found a large cyst, which filled the whole orbit behind the eye; and so part of the cyst was left to slough off with the dressings. The whole eye being cut out, I filled the wound with lint, &c. and in three days I removed the dressings, together with a great quantity of sanies, which were daily renew'd, and the part of the cyst, which was left behind, slough'd off the second day. The cure went on with  
success,

success, and, in a month from the operation, was completed; and she remains free from pain from that time to this.

*P. S.* I must mention what seemed remarkable upon the case: when the woman was put to bed, we came to observe the state of the eye, which appear'd a little bigger than the other; and having cut through it, we found the humours very much confused: the aqueous humour was not so clear as usual, the crystalline less solid and transparent, and the vitreous almost reduced to a liquid state. The cyst was very strong and elastic, and had a cavity large enough to contain a large hen's egg.

*V. A Supplement to the Account of a distempered Skin, published in the 424th Number of the Philosophical Transactions. By Mr. Henry Baker, F. R. S.*

Read Jan. 23, 1755. **I**N the year 1731, a lad, fourteen years of age, was brought by his father from Euston-Hall, in Suffolk, and shewn to the Royal Society, on account of his having a cuticular disorder, of a different kind from any mentioned in the histories of diseases.

The extraordinary case of this boy was drawn up by Mr. John Machin, at that time one of the Secretaries of the Royal Society, and was published in the Philosophical Transactions, N<sup>o</sup> 424.

As more than four and twenty years are now past since this account was given, and the person therein mentioned is still alive, and was lately shewn at London, by the name of the Porcupine-man, with a boy in the like condition, both which I saw, and examined; some farther knowledge of him may not, I hope, be thought undeserving the attention of this Royal Society.

His name is Edward Lambert. He is now forty years of age; a good-looking, well-shaped man, of a florid countenance; and, when his body and hands are covered, seems nothing different from other people. But except his head and face, the palms of his hands, and bottoms of his feet, his skin is all over covered in the same manner as in the year 1731, which therefore I shall trouble you with no other description of, than what you will find in Mr. Machin's account above-mentioned; only begging leave to observe, that this covering seemed to me most nearly to resemble an innumerable company of warts, of a dark-brown colour, and a cylindric figure, rising to a like height, and growing as close as possible to one another; but so stiff and elastic, that, when the hand is drawn over them, they make a rustling noise.

When I saw this man, in the month of September last, they were shedding off in several places, and young ones, of a paler brown, succeeding in their room, which, he told me, happens annually in some of the autumn or winter months: and then he commonly is let blood, to prevent some little sickness, which he else is subject to whilst they are falling off. At other times he is incommoded by them no otherwise, than by the fretting out his linen, which, he says, they  
do

do very quickly : and when they come to their full growth, being then in many places near an inch in height, the pressure of his clothes is troublesome.

He has had the small-pox, and been twice salivated, in hopes of getting rid of this disagreeable covering ; during which disorders the warting came off, and his skin appeared white and smooth, like that of other people ; but, on his recovery, soon became as it was before. His health at other times has been very good during his whole life.

But the most extraordinary circumstance of this man's story, and indeed the only reason of my giving you this trouble, is, that he has had six children, all with the same rugged covering as himself : the first appearance whereof in them, as well as in him, came on in about nine weeks after the birth. Only one of them is now living, a very pretty boy, eight years of age, whom I saw, and examined, with his father, and who is exactly in the same condition, which it is needless to repeat. He also has had the small-pox, and during that time was free from this disorder.

It appears therefore past all doubt, that a race of people may be propagated by this man, having such rugged coats or coverings as himself : and, if this should ever happen, and the accidental original be forgotten, 'tis not improbable they might be deemed a different species of mankind : a consideration, which would almost lead one to imagine, that if mankind were all produced from one and the same stock, the black skins of the negroes, and many other differences of the like kind, might possibly have been originally owing to some such accidental cause.

Mr.



Mr. George Edwards, Librarian of the College of Physicians, having lately drawn, and etched on a copper-plate, the hand of this boy, in such manner, as to shew the palm free from these excrescencies, and its other parts covered with them; and also a company of the excrescencies, as they appear where largest; a copy of the said plate, which I was favoured with by him, is now presented with this account.

January 23, 1755.

VI. *An Extract of the Substance of Three Letters from Isaac Jamineau, Esq; his Majesty's Consul at Naples, to Sir Francis Hoskins Eyles Stiles, Bart. and F. R. S. concerning the late Eruption of Mount Vesuvius.*

A larger Extract of these Letters was read Jan. 9, 1755.

IN the first of these letters, which is dated December 7, 1754, Mr. Jamineau acquaints Sir Francis, that they were then in the third day of an eruption of Vesuvius, which already far exceeded the last in 1751, and bids fair to equal any of those since the burial of Herculaneum. It is some time that they have expected the present, from foregoing circumstances, similar to, though more extraordinary than, those of former eruptions. Mr. Jamineau himself was witness to the following ones. In April last, the fire issued from one end of a hillock, in the shape of a crescent, within the crater, to which you descended from the upper

upper rim of the hill. At that time the descent was about eighty feet, which Mr. Jamineau measured himself. On his second visit, in September, the crescent was turned to a cone, but much higher than before, being increased in proportion to the fire, that now discharged, by frequent explosions, thousands of stones on fire. On a third visit, in the middle of October, the cone seemed lower, which was owing to the rising of the bottom of the cup, whose depth from eighty feet was decreased to fifty. The lava was actually running in many places; and where it was not, the fire was universally visible within a foot or two of the surface. They descended to the bottom, and approached the running lava, whose progress was so slow, that they sat a full quarter of an hour within three yards of it, in its course, before they were obliged to shift their places. Such of the guides, as had shoes on, ran over the very matter as it was proceeding, and it was with difficulty, that Mr. Jamineau prevented one of those, who had not, from doing the same thing. This he was induced to do from a passage of an inscription at Portici, describing the course and effects of the lava, *Si corripit, actum est, periisti*. Whence he was apprehensive of the effects of the matter's adhering to this man's feet; for he was under no apprehension of his sinking in, since the upper coat of this substance was so hard, as to bear the throwing the largest stones two of the company could lift, with very little impression, at the same time that the bottom was penetrable, with ease, to a small stick they thrust into it. The running of the lava within the crater increased daily, so that in a month's time the cup was filled within twenty-five

feet of the top. On which one Mr. Vernet (on whose judgment Mr. Jamineau could rely) pronounced, that there must be soon an eruption from the sides, or an overflowing at the top. Accordingly, on tuesday the third of December, at night, after a little shaking, which was not felt above two or three miles off, an opening burst on the eastern side of the mountain; but the matter soon ceased running from this orifice, and burst out from a much greater one, about two hundred yards below it. From this there afterwards overflowed no matter; but the lava has run from it within, though very near, the surface, to a third furnace, whence the liquid fire now pours out. This chanel of fire, after falling from the third furnace, with great fury, a few yards, is covered by the hard exterior surface of the lava, which cools and incrusts on its surface, as its course is on a level or gently declining ground, till it comes within ten yards of the top of a steep declivity. Here the fire collects as in a reservoir, to supply a cascade, which rushes down from thence in a channel of more than twenty feet wide, and about two hundred yards in length, with a fall of at least fifty feet, divided upon such length. After which the stream is less rapid, but grows wider, and has already forced its course for four miles from the source, where it affords a very different scene from what it presented from its first eruption. For there it runs over a country already destroyed; the cascade looks like melted gold, and tears off large bodies of old lava, which float down the stream, till the intenseness of the heat lights them from the bottom. But, in the lower country, the chanel is divided into lesser streams, running  
with

with less rapidity; whence, notwithstanding its slowness, it drives the strongest stone fences before it, and from lighting the trees, like torches, affords a most extraordinary, though dismal and pitiful, spectacle.

The second letter is dated December 14, 1754, and only gives an account of the continuance of this eruption.

The third letter, which bears date the 17th of the same December, says, that Vesuvius increases his glory and desolation; and that the stream of fire mentioned in the first letter is a mere rivulet, when compared to a second, of which Mr. Jamineau proposes to give an account in a subsequent letter.

*Extract of another Letter from Mr. Jamineau, the British Consul at Naples, to Sir F. H. Eyles Stiles, dated December 21, 1754.*

Read Jan. 23, 1755. **I** Should wish to say something of the main branch of the fiery river, which I never saw till two days since; but the departure of the post confines me to say this only, that the lesser, which I saw before, is a small trout-stream compared to this, which sets off in a cascade of a mile's length, and, though rather with a less declivity, is equally rapid, from the greater quantity of matter rushing down it. The breadth was about sixty feet at the top; but by having melted down an island, that divided its stream about two hundred yards in the

fall, which I will answer for its having done now, by the progress I saw made towards it for the hour that I staid; I say, now, the breadth in that place must be above a hundred yards.

*Extract of a Letter from the same Gentleman, to the same, dated December 28, 1754.*

Read Jan. 23, 1755. **V**esuvius runs but with one, though that the most considerable stream of fire: he has also changed his note at top, and is now more angry than ever, throwing up stones and matter in greater abundance.

VII. *An Account of the Species of Plant, from which the Agaric, used as a Styptic, is prepared. By Mr. William Watson, F. R. S.*

London, Feb. 1. 1755.

Read Feb. 6, 1755. **I** lately acquainted the Royal Society, that I had some doubts, arising from the pieces I had seen, whether the agaric sent from France, and applied as a styptic after amputations, was the *fungus in caudicibus nascens, pedis equini figurâ*, of Caspar Bauhin; or the *fungus coriaceus quercinus hæmatodes* of Breynius, of which I gave a short history. In order therefore to be confirmed in this matter, I wrote to our worthy brother M. Clairaut, of Paris, to lay some queries, I drew up, before M. Bernard de Jussieu, and M. Morand, both members

members of this Society, and excellent judges of this subject. M. Clairaut, since our last meeting, has been so kind as to transmit to me their answer; by which it does appear, that this is prepared from a plant, which is called by the botanists

“ *Agaricus pedis equini figurâ. Inst. Rei. Herbar.*

“ *Fungus in caudicibus nascens, pedis equini figurâ.*

*C. B. Pin.*

“ *Fungus durus arborum, five igniarius. Park. Theat.*

“ *Fungi arborei ad ellychnia. I. B.*

“ *Fungi igniarii Cifalpini et Tragi.*

“ *Boletus acaulis pulvinatus lævis, poris tenuissimis.*

*Linn. Flor. Suec.*

It is the agaric employed for the amadou; and Mr. Broffart, who first brought this preparation into practice, conceives, that that, which grows upon old oaks, which have been lopped, is the most valuable; that it should be gathered in August or September, and be kept in a dry room.

The way of preparing it is to take off with a knife the white and hard part, till you find a substance so soft, as to yield under the finger, like shammy leather. This is to be divided into pieces of different sizes and thickness: beat these with a hammer, to give them a still greater degree of softness, so that they may be easily torn with the finger.

Mr. Morand thinks, that the agaric, which when growing is of a greyish colour on the outside, is better than that which is white.

W. Watson.

VIII. *An Account of a Mountain of Iron Ore, at Taberg in Sweden, in a Letter to Mr. Peter Collinson, F. R. S. By Peter Afcanius, M. D. Translated from the Latin by Mr. Emanuel Mendes de Costa, F. R. S.*

Dear Sir,

Read Feb. 6, 1755. **I** Herewith fend you a short but accurate account, and explanation, of the drawing I shewed you, when I had the pleasure to view your collection of minerals, and we discoursed on them. The subject is not unworthy your curiosity and learning, which I have long heard of, but which I now have the pleasure to be acquainted with. This short description I have extracted from the journals of my travels; and it may perhaps be of utility to some future traveller, who may choose to visit the place. Permit me to intitule it,

*A Description of the Mountain, which is intirely composed of Iron Ore, at Taberg in Smalandia in Sweden.*

The mines of Sweden are justly esteemed superior to the mines of most other countries; and those of iron are the most famed. Among the most curious of the latter is that of Taberg, if, with propriety, it can be called a mine. The Swedish iron is, and has always been, carried to most parts of Europe, and

is preferred to all other iron, for many reasons, as daily experience demonstrates.

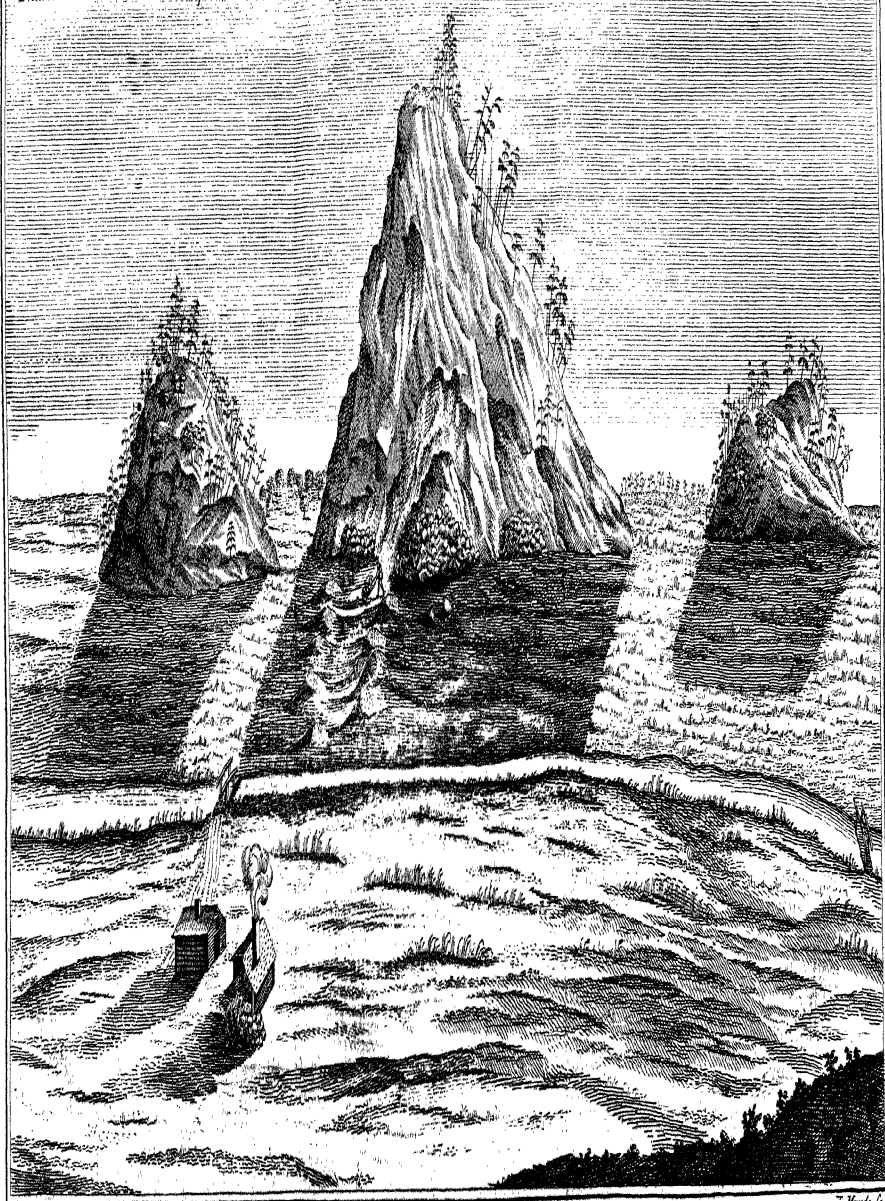
Most, but not all, iron ores are attracted by the loadstone: the reason seems to be for those, which are not attracted, that there are no native particles of iron, or that the ore is not sufficiently mineralised in them. The Swedish ores are almost generally attracted by the loadstone; and from that property, not without reason, many skilful mineralists account for the excellency of the Swedish iron. This mountain is situated in a sandy tract of land, of which the sand is extremely fine. Opposite to it is a valley, through which a small river flows; its perpendicular height is above four hundred feet; its circumference half a Swedish league, or three English miles. The whole mountain is one mass of rich iron ore, and even in some parts is mixed with particles of native iron. Wallerius's Mineralogy Species 254, Variety 2d. synonyms it *Ferrum Mineralisatum*. S. *Minera ferri nigricans solida, Magneti amica*; and Linnæus, *Systema Naturæ*, p. 176. No 9. *Ferrum intractabile cinereo-fuscum, punctis nitidis*; in which he contradicts this ore being attracted by the loadstone, though all the specimens I have tried have been always attracted by it. The broken pieces glitter with shining particles, sometimes placed in a scaled, and sometimes in a striated manner. The neighbouring small rocks are of a greyish stone (*saxum purum*). About two hundred years ago (for so long have they work'd on this mountain) they blew up the masses of ore; yet the mountain appears very little diminished, except in the laves or hollow places, which are at the foot of the mountain,



tain, opposite to the valley. By what has been said, it is to be understood, that the iron ore does not lie in regular strata, as in other places; neither is the ore every-where of equal goodness. There are many perpendicular as also horizontal fissures all over the mountain, which are filled with the same sand, reduced to a kind of fine mud-like paste; and in no part whatever is it impregnated with the least particle of the iron ore of the mountain, but is of the same purity and nature, as it is found on the sea-beaches, from whence often, by its lightness, it is carried by the winds, and covers and destroys whole tracts of land, as it happens in Scania, Seeland, and Holland. In the interior fissures of the mountain, bones of animals, as of stags and other kinds, are frequently found imbedded in the sand. No ore is found beyond the foot of the mountain, nor on the neighbouring plain; so that it appears, as if the mountain had been artificially laid on the sand, for it has no roots, or, like other mountains, its substance does not penetrate the ground. The ore breaks easily, and what is broke from the sides of the mountain readily falls to the foot of it; while in other mines the ore, with great trouble and costs, is dug from the bowels of the earth. The only inconveniency which happens here is, that the sand, which is lodged in very quantities in the fissures, when the ore is blown up, falls with it to the foot of the mountain, and buries or covers it, which they are forced dig away again: on which account they always blow up the ore from the bottom of the mountain upwards, for the greater ease of the miners, and to hinder the heaping of the sand at the bottom. They then carry  
the

the ore to the neighbouring furnaces, where being roasted, and broken small, they mix it with limestone and powder'd coal, and smelt it into iron.

These particulars, attentively considered, make this mass or mountain of iron ore, not only a very curious production of nature among the Swedish natural rarities, but perhaps among those of the globe. The generation and site of this mountain are extremely difficult to explain: the most probable system seems to have recourse to an inundation; but as the mountain is situated in a high and mountainous tract, and is near forty Swedish leagues distant from the sea, no other inundation but the universal deluge can be brought to account for it. Perhaps it might be conjectured, that, by the violent and rapid motion of waters, this mountain, which before was intirely buried in the sands, was uncovered and left bare. This would indeed be probable, if the whole country about it had been plain; but on the contrary it is a very rugged tract, nor are there in the adjacent parts the least vestiges of the sand being carried or diffipated elsewhere. It therefore to me seems more reasonable to attribute its origin or formation to subterranean causes, which by violent shocks changed the whole face of that region, and left the mountain thus elevated and bare; because we have no examples, to lead us to think (if we draw a conclusion from similar cases) that this mountain became thus mineralized in every part of it, when bare or exposed, as we now find it. This alone is certain, that it was once quite buried in the sands: the other particulars we remain ignorant of. This is more probable, as it appears more conformable to reason than other





luxuriant imaginary systems, which rather force than elucidate, and very little agree with the laws of nature. Who hitherto has ever rightly explained the origin of mountains? We perhaps know some particular causes, but how can we draw from them general conclusions? The bones of animals, which are found in the interior fissures of the mountain demonstrate it to be formed by a ruinous cause. This suffices not to explain, but only to illustrate, the subject. In the annexed *Plate I.* *A* is the mountain Taberg. *B, B, B,* the heaps of broken ore. *C, C, C,* the sand brought forth from the fissures. *D, D,* the neighbouring stony rock; and *E,* the miners houses. I am,

S I R, &c.

Peter Ascanius.

IX. *An Account of an extraordinary Case of a Child.* By Mr. Richard Guy, Surgeon.

Read Feb. 13.  
1755.

**A** Child near seven years of age, the daughter of an eminent tradesman in Bishopsgate-street, having languished, for near twelve months past, of a supposed dropsy, and undergone the most skilful treatment of several eminent physicians unsuccessfully, died in an emaciated state.

By desire of the parent, I opened the body, expecting to find water, but, to my great surprize, there appeared as follows: A large round solid substance, shaped in the form of an egg, weighing fourteen

fourteen pounds two ounces and an half, of the adipose cellular consistence; some parts of it being more brawny than others. On dividing it through the center, were found several little cists, containing a meliceratous fluid; the whole seemed envelop'd in a membrane, which I apprehend to be the omentum, but the extension, from so large a body contained in it, had made it almost loose its reticular appearance. It was surrounded with many small blood-vessels, but no considerable ones. It adhered to the peritoneum, the back-bone, and almost all the internal cavity of the abdomen, resting the large end in the pelvis, and thereby greatly compressing the bladder and ureters. The intestines were all crowded together on the right side, in as small a compass as could possibly contain them. The intestine colon passed round the lower part, in the form of an S, which adhered likewise: it also envelop'd the right kidney, which appeared something bigger than the other; and, upon dividing it, I found small stones, not exceeding the size of a large pin's head. The other kidney did not adhere to this substance. The small end pressed upwards against the diaphragm, so hard, as to force the heart close under the left clavicle: the lungs were so confined, as to render only one lobe capable of respiration; the others appeared as in a still-born child. The liver, gall-bladder, and spleen, were as in health; the intestines the same; the mesentery was much extended with blood; the matrix and ovaria as in their natural state; and no other parts, that I could discover, affected. I could not discover, on dissection, any nuclei, that might particularly supply, or give rise to, this enormous substance.

The child died the 5th instant. I have preserved the substance at my house in Mark-lane.

Feb. 6, 1755.

Richard Guy, *Surgeon.*

X. *Extracts of Two Letters from Mr. James Latterman, Student in Physic and Surgery, to Dr. Schloffer, now residing in London, concerning the Effects of the Agaric of the Oak, after some of the most capital Operations in Surgery. To which are added, Some remarkable Experiments made upon the Arteries of Horses, with the Powder of the Lycoperdon, or Lupi Crepitus; by Monsieur La Fosse, Farrier to the King of France. Communicated by Mr. Joseph Warner, F. R. S. Surgeon of Guy's Hospital.*

*Extract of the First Letter, dated at Paris, January 15, 1755.*

Read Feb. 13. 1755. **D**R. Latterman informs Dr. Schloffer of his having made application to Monsieur Andouillet, Monsieur Moreau, and to many other surgeons of note, to know upon what subjects, and how often, they have made use of the agaric of the oak. Monsieur Andouillet informs Dr. Lat-  
terman,

terman, that he had applied it after six amputations, and successfully; three times after amputations of legs above knee; and three times after amputations of legs below the knee.

Monfieur Moreau, furgeon to the Hotel Dieu, informs Dr. Latterman, that he had applied the agaric of the oak, after feveral amputations of legs below the knee, and after feveral amputations of the arm both above and below the elbow, and every time with fuccefs: that their method of applying it is by placing pieces of the agaric upon the mouths of the veffels, and upon them doffils of lint.

*Extract of the Second Letter, dated at Paris,  
February 5, 1755.*

Read Feb. 13, 1755. **D**R. Latterman informs Dr. Schloffer, that Monfieur Andouillet, furgeon of the Charitè, has lately amputated a leg above the knee. He applied the agaric of the oak in the ufual manner, and no bleeding at all has fince happened. Two days after the operation, the Doctor faw him remove the dreffings, and the whole of the agaric: the mouths of the veffels appeared quite clofed, and he made no farther ufe of the agaric to the wound. The patient has a fair profpect of doing well; and the wound appears properly digefted.

Dr. Latterman then proceeds to give Dr. Schloffer an account of fome experiments made upon the principal arteries of horfes, after being divided. Dr. Latterman obferves, that the virtues of the powder of the lycoperdon, or the lupi crepitus, have been



been known for some time; but that Monsieur la Fosse has made them more conspicuous by his experiments. His first observation is this, that, upon applying it upon the mouths of the largest arteries, when divided, the bleeding has ceased in a few minutes, and that the mouths of the divided arteries have healed up without any farther discharge. His second observation is, that in twenty-four hours after the application of this powder, a thin pellicle or skin is formed upon the mouths of the divided arteries, and that within the vessels is found a small plug of congealed blood. His third observation is, that the pulsation of the artery is to be seen in a very distinct manner at the extremities of the vessels. His fourth observation is this, that the coagulated blood is of a conical figure, whose basis is at the mouth of the vessel, and its apex in an opposite direction.

Dr. Latterman says, that all these curious experiments have been verified by the commissaries of the Academy of Sciences, who, upon observation, have found them all agreeable to truth.

XI. *A Letter from James Parsons, M. D. F. R. S. to Thomas Birch, D. D. Secret. R. S. concerning the Use of Lycoperdon, in stopping Blood after Amputations.*

S I R,

London, Feb. 13, 1755.

Read Feb. 13,

1755.

**T**HE laudable endeavours of several gentlemen of our learned Society, as well as of Paris, have been exerted strenuously

ously in ascertaining the quality of the agaric with respect to stopping the blood after amputations ; and it appears, that in many instances the success answered according to expectation. There are still, however, various objections made by some to its efficacy ; and some doubts concerning the inefficacy of its power upon the more considerable arteries especially.

And as it were to be wished by every humane person, who is called to undertake those painful operations of the amputation of limbs, which his duty, however arm'd with compassion, authorises, that any means could be found whereby part of the patient's sharp sensations might be taken off ; so every attempt towards this laudable end will be very warmly received, by those whose sanction has heretofore often encouraged every essay advanced for the public good.

It is not unlikely that, at present another, even a more certain method is found for stopping the blood in the most considerable arteries after amputations ; which, if the testimonies of the Royal Academy of Sciences can be relied on, cannot admit of either hazard in the application, nor delay in its effects : nor is there the least need of the assistance of the tourniquet to restrain the blood while the remedy is applied ; where pain is avoided, and yet the necessary suppuration goes very happily on towards perfecting the cure.

I therefore shall hasten to the history of the remedy, and recommend it to our surgeons to make the experiment on quadrupeds of the larger kinds, as the author of this invention has done, before it is attempted upon human bodies ; and must call it an  
invention.

invention, because though authors have mentioned it as good to stay hæmorrhages, yet none before him, that I know of, has made use of it in the case of amputation.

Monf. la Fosse, Farrier to the King of France, has published a book lately, which treats of some diseases in horses, which I happened to see yesterday morning, wherein he takes occasion to give a particular detached piece upon a remedy for stopping blood in wounded arteries; of which the following is an extract; and also of the declaration of the Academy of Sciences upon the experiments made with it.

The subjects he chose for this purpose were horses; and, in the first experiment, he laid the temporal artery bare, and divided it half-way transversely, from which the blood issued with great violence: he immediately applied the powder of the crepitus lupi, or puff-balls, a very common production in our fields, lycoperdon, by the botanic writers; which he confined only with the palm of his hand to the part, and the blood stopped. He prick'd in the same manner an artery in an horse's leg, and stopped it in the same manner.

To make his next experiment, he cut off the fore-leg of a horse, near the breast, and applied to the stump this powder, confined only by a piece of bladder, and the blood stopped; tho' the horse struggled a great deal, for he was flung to make the amputation. The tail of the same horse was cut off near the body, and the four arteries were immediately stopped in the same manner.

Four days after, this horse was killed, and *Monf. la Fosse* dissected the arteries divided before; and found in every one a membrane almost transparent, with a grume in form of a cone, which closed up the mouth of the artery very exactly; the basis of which was outward, and the apex turned inward; that he could observe the motion of this membrane and cone correspond with the arterial pulsation, advancing and receding, while the horse was alive; and that in every subject upon which he made the experiment, the same effects were exactly produced, without the least variation. And further, in order to see whether the suppuration, consequent to the amputation, in the progress of a cure, might not open the arteries again, he let the strongest horse live till a perfect suppuration came on, and found that it was not capable of hindering the cicatrizing of the arteries.

These experiments, among others, were laid before the Royal Academy of Sciences; and they appointed *Monf. Bernard de Jussieu* and *Monf. Bouvard* to examine the truth of these facts; and accordingly *Monf. la Fosse* cut off the tail of a young mare, in their presence, applying this powder, and a bladder over it; and took off these dressings in a quarter of an hour after. Three of the arteries were absolutely stopped, but the fourth bled freely; to which he applied some of the powder with his fingers only, and the blood in this last artery was perfectly stopped in six minutes.

Again; eight days after, the thigh of this young mare was cut off ten inches above the ham; and in order to try whether the powder of Armenian bole

would not have the same effect, they applied it to the stump, and secured it with proper bandage: and after two hours and an half, the blood sprung out with the same impetuosity; when Monf. la Fosse applied the lycoperdon powder, which he held on with his hand alone, for only six minutes: he took away his hand, and no artery bled but the crural; but the stream of this was very small, by means of the powder; which they let alone for six minutes, and yet it did not increase: then this gentleman put a pinch of the powder to this bleeding vessel, and the blood stopped in four minutes before them. In all these operations the same cone and membrane were formed; for they dissected them all afterwards, and this effect of the medicine was ever constant.

These were the facts strongly attested by the committee of the Academy mentioned above; which, I think, as strongly deserve the notice of the world; and if the same effects are produced upon human bodies, the painful tying up the arteries, and the hazard or uncertainty of the agaric, will be fully compensated in this noble powder.

It is said by those two gentlemen, that this powder was not quite unknown before: true, I know that, in countries where I have travelled, the common people know it to be good for stopping blood, and accordingly use it for bleeding of the nose, and accidental cuts in any part; and authors have indeed mentioned it as good in the same intentions; but I do not know of any who have brought it to the test in amputations, where its use is very capital, and its application so easy and simple, and when also every field almost, during the autumn, produces these balls.

Mr.

Mr. Ray, in his history, ascribes that virtue to this powder ; and also says, it powerfully dries up foul ichorous ulcers ; and many others commend it as a styptic besides.

This, Sir, is what I thought necessary to lay before the Society, from the same motive that I am sure would excite every member, if opportunities offered. I am,

S I R,

Your most humble servant,

J. Parsons.

XII. *An Account of the State of the Thermometer, on the 8th and 9th of February 1755, in a Letter to John Canton, M. A. F. R. S. from Henry Miles, D. D. F. R. S.*

Dear Sir,

Read Feb. 13, 1755. **I** Thank you for the register of your thermometer on the late cold days, and have inclosed that of mine, on the same times ; which, if you please, you may lay before the Royal Society, with the following remarks.

The cold on the 8th instant, especially at midnight, was extraordinary, if it be considered, in how short a space of time it increased to that degree you mention. And that this may appear, I have prefixed

to the register of my thermometer, in the annexed paper, the state of it on the 7th, on which day, tho' the wind blew very strong from the east, the cold exceeded the degree of freezing-point but very little. Some allowance is indeed to be made for the thickness of the atmosphere.

The lowest state of my thermometer was on the 8th day, at 7 h. *a. m.* when it had fallen to  $17\frac{1}{2}$ ; though it appears to have been unusually cold throughout the day, by the register.

It is probable, that the crisis was at or about the time of your midnight observation; for the wind shifted to the S. E. and the thermometer indicated a considerable abatement of the cold several hours before day. As you have mentioned the difference between the height of the thermometer placed against the house, and that of one in the middle of your garden, I would inform you, that I have formerly found about the same difference between them, when I have made the same trial.

A thermometer within my chamber, near another placed on the outside of the window, continued falling till the ninth day; when at 6 h. *a. m.* it stood at  $25\frac{1}{2}$ . At 8 h. *p. m.* when that without was at 38, this within was no more than 28, at which time water was congealed in my chamber, and it froze all night, and part of the next day, in the same; tho' the window was covered thick with small drops of water on the outside, even at 8 h. *p. m.* so that here was a great increase of cold within, while there was a very considerable abatement of it abroad. So little dependance can we have on the indications of the temperature of the air by thermometers placed in a house.

I only

I only add my ardent wishes, that a number of gentlemen, residing at a distance from each other, would agree to make regular observations of the weather, with instruments of the same construction, and framed by the same hand. Their registers would be worthy to stand on record; and, by comparing them, an improvement might be made in the natural history of the air, so justly complained of, as being very defective: and from thence might ensue benefit to the public in several respects. I am,

Dear Sir, to you

Tooting, Feb.  
13, 1755.

and the Royal Society,

a faithful humble servant,

H. Miles.

1755. Mr. Canton's register of his Thermometer, in Spital-square, London.      The register of my Thermometer, at Lower-Tooting, in Surrey.

				Feb. 7th . . . . 6 <sup>h</sup> . a. m. 31.	
				2 <sup>h</sup> . p. m. 33.	
				. . . . 8 <sup>h</sup> . p. m. 30.	
				cloudy all day, and W.	
				high at E.	
Feb. 8.	2 <sup>h</sup> . p. m.	27		7 <sup>h</sup> . a. m. 17 $\frac{1}{2}$ .	2 <sup>h</sup> . p. m. 26 $\frac{1}{2}$
	8 <sup>h</sup> . p. m.	20		cloudy W.E.	
{ In the middle of the garden				8 <sup>h</sup> . p. m. 19 $\frac{1}{2}$	
{ At midnight				clear.	
{ In the middle of the garden					
Feb. 9.	At 8 <sup>h</sup> . a. m.	- - -	21 $\frac{1}{2}$	At 6 <sup>h</sup> . a. m. 23. not lower	
	2 <sup>h</sup> . p. m.	- - -	30 $\frac{1}{2}$	after, cloudy. W. S. E.	
	8 <sup>h</sup> . p. m.	- - -	33 $\frac{1}{2}$	2 <sup>h</sup> . p. m. 33 $\frac{1}{2}$ . very cloudy	
				W. S. E.	
				8 <sup>h</sup> . p. m. 38. f. cloudy all.	
				W. S. E.	



**XIII. *An Account of some Cases of Dropsies, cured by sweet Oil: In a Letter from William Oliver, M. D. F. R. S.***

S I R,

Bath, Feb. 10, 1755.

Read Feb. 20, 1755. **I** Cannot recollect whether Miss \* \* had been tapp'd, or not, when you did me the favour to call at my house. But I dare say, an account of her proceedings since will not be unacceptable.

Mr. Pierce took from her eleven pints of water. As soon as the bandage could be loosened, Dr. Hartley and I examined the state of her belly. The epigastric region was quite emptied; but we found a great fulness, which extended itself on each side the inguen, towards the back. We put her upon a very spare dry diet, and allowed her but a quarter of a pint of liquids in the twenty-four hours. But tho' her urine much exceeded in quantity what she drank, the swelling increased, and we feared the belly would soon fill again. A lady, who was with her, told us, that, just before she left London, she had heard, that two persons had been cured of confirmed dropsies by being anointed, morning and evening, with common salad oil, which was rubbed into the whole abdomen, for an hour at a time, with a warm hand. We could not refuse the trial of so innocent a method. The Iâtraleiptæ began their operation. About the third day of anointing, the urine was considerably increased, and continued to be so. The fulness gradually decreased, and in a fortnight's time was quite gone.

gone. Her appetite, digestion, and sleep, grew natural, and she recovered flesh, strength, and spirits. About six weeks after her first anointing, her menses appeared, and at the end of the next month she had a regular return, of good colour, and in sufficient quantity. I saw her at the public room last week, in as good health as I ever remember her to have enjoyed.

You may be sure this recovery was much talked of, and set all the hydropics a rubbing.

A man, aged fifty-five, from hard-drinking, and many wrong methods of cure, had been cachectic fifteen years, and had often the symptoms of jaundice and dropsy. Half a year ago, his belly, legs, and thighs, swelled to an enormous size. He was with difficulty moved from his bed to his chair, and was given over, as a person in an incurable dropsy. About three weeks ago, he began to anoint. After three or four days rubbing, his urine was greatly increased; and in a fortnight, his belly, thighs, and legs, were wonderfully decreased, and I saw him a few days ago walking about the town, whereas before he could not move a joint.

A woman of seventy years of age, of a thin habit, who got a livelihood by carrying cakes about the town, fell into an ascites. Her belly was so greatly distended, that she was obliged to quit her business, to confine herself to her house, and for the most part to her bed. She anointed. Her urine soon increased in quantity, and continued to do so. She was at my house this week, as lank, as she said, as a maiden, and in as good health as she had enjoyed for many years.

These

These cases are, I think, sufficient to encourage farther trials. I am, with true respect,

S I R,

Your most obedient humble servant,

W. Oliver.

XIV. *Observationes Eclipsium Satellitum Jovis habitæ Ulissipone in Regio Collegio Beatissimæ Virginis à Necessitatibus nuncupatæ, à Joanne Chevalier Cong. Oratorii & Regalis Societ. Lond. Socio, Anno 1754.*

Read Feb. 27,  
1755.

**D**IE undecima Januarii, telescopio magno Gregoriano, long. 6 pedum dimidio, nocte clarissimâ, observavi immersionem secundi Satellitis horâ postmeridiana temporis veri, 9<sup>h</sup>. 4'. 3". Hac, ac sequentibus diebus, clarè conspiciebantur fasciæ duæ in disco Jovis.

Die decima-quinta Januarii, eodem telescopio, cælo etiam claro, observavi immersionem primi Satellitis horâ postmeridianâ temporis veri, 11<sup>h</sup>. 23'. 58".

Die decima-octava Januarii, eodem telescopio, observavi immersionem secundi Satellitis horâ postmeridianâ verâ, 11<sup>h</sup>. 35'. 30".

XV. *An Account of those malignant Fevers, that raged at Rouen, at the End of the Year 1753, and the Beginning of 1754. By Monf. Le Cat, M. D. Professor of Anatomy and Chirurgery at Rouen, F. R. S. Member of the Royal Academy of Sciences at Paris, and perpetual Secretary of that at Rouen.*

Read Feb. 20, and  
March 6, 1755.

**A**BOUT the end of November 1753, a malignant distemper broke out in this city; the ravages of which continued during the subsequent months of December, January, and part of February.

I was induced, by the noise it occasioned throughout Europe, to treat of it in a particular manner.

In order to which, I shall give a history not only of this last epidemical disease, but also of that of the preceding years, to which it is nearly related.

The medical gentlemen, who have practised in this city from the beginning of the present century, have assured us, that, for the last thirty years, this country has been more subject to malignant fevers than it had ever been before; and that the greatest part of them have been accompanied with miliary eruptions. I should be tempted to fix this epocha in 1723, and 1724, because the first of these years was excessively dry, the rain at Paris amounting to no more than seven inches eight lines, (whilst the mean year comes to nineteen), and the year 1724 had only

twelve: whilst the year 1725 produced more than seventeen inches and an half, which should cause a temperature nearly approaching to the mean quantity, which may be looked upon as the most healthy.

I observed in 1736 and 1737 certain gangrenous sore throats, which chiefly attacked children: they appeared again in 1748, in young persons of the first distinction, not only at Rouen, but also at St. Cyr, near Versailles, and at Paris. Persons of a certain age were also seized with it, not only in town, but in the country; and in some the tongue alone was the seat of the gangrenous eschar. In the same years 1737 and 1738, there was a great number of malignant peripneumonies, of that kind called pituitous. The lungs of these subjects, many of which I open'd, were become schirrous; and the patients perished for want of being able to admit air into them, as if they had been strangled. Some of them most earnestly begged of me to open their breasts, imagining that a new vent would give them breath.

In 1739 we had, at the Hôtel Dieu, continual fevers, with frequent faintings; and the patients, without any other violent symptom, died in six or seven days. I found small abscesses in the substance of their hearts, near the auricles. Nothing remarkable happened from 1739 to 1743, but that the finest, longest, and driest summer I ever knew in Normandy, produced epidemical bloody-fluxes, which grievously afflicted both this city and the whole country round about. These fluxes were preceded by great lowness of spirits, attended with violent colics, and a sharp fever: the pulse small, the mouth and tongue foul, a nasty  
taste

taste in the mouth, and frequent nausea; and whenever a hiccup came on, death was not far off.

The principal feat of this distemper was in the large intestines; though sometimes the small guts and stomach had their share. In one, whose name was Coulon, who voided pure blood a little before his death, I found a great portion of the intestinal canal full of blood, the villous coat being much swell'd, and greatly inflamed: and, putting it in water, one might easily discern, with a magnifying-glass, a great number of red points, which appeared to be the mouths of the vessels, which poured out the blood found in the intestines.

One Le Fevre had blood discharged even up to the stomach; and the inner membrane of this organ, towards the pylorus, was in the same condition with that of the great intestines of the foregoing patient. The duodenum, jejunum, and the beginning of the ileum, were sound; the end of the ileum was inflamed, and the large intestines were gangrened.

In one, called Saracin, the same intestines were all mortified; the cæcum, and half the colon, were as large as a stomach distended with wind. Their canals were full of a bloody matter, and their inward membrane separated very easily. The gangrene seemed particularly to affect this coat. The stomach and small guts were sound; nevertheless his death was preceded by the hiccough.

In some others, the gangrene had seized all the coats of the intestines; and sometimes these canals were so far pierced by the eschars, as to let the fæces pass through into the cavity of the belly. And in

some the bladder itself partook of the disorders, that were observed in the great intestines.

A few bleedings at first, cooling liquors, as whey, chicken-water made into an emulsion, emollient clysters often repeated, and paretics given properly, and in small quantities, were the most sovereign remedies for this disease. Purgatives were generally hurtful.

However, ipecacoanha succeeded with some; and an English pupil of mine, Mr. George Ross, made very successful trials with boluses of vitrum antimonii ceratum.

Whenever blood was taken away in an over great quantity, the patient in three or four days fell into the agonies of death. Anodyne drops given too freely, instead of quieting, occasioned restlessness, and increased the fever and inflammation.

I was myself struck with this disease, as if with lightning, and passed, in a few hours, from a good state of health into a sinking and insensibility, which indicated a gangrene coming on, and the utmost danger. Two bleedings, close upon one another, brought me to myself; but my insensibility was succeeded by the usual colic and flux, which was the principal distemper: then one half ounce of diacodium freed me from this painful and dangerous condition, as speedily as the infected air had thrown it upon me.

In the following season, and even in the year 1744, when this distemper prevailed no longer as an epidemical one, there happened some very extraordinary circumstances.

On the 30th of November 1743, the wife of a writing-master, being of a robust habit of body, and in perfect health, was suddenly seized with a violent colic in her stomach, and died in three hours. I found three gangrenous places at the upper orifice of the stomach. I doubt whether ever any distemper could have deserved the name of a plague more than this, if it had been epidemical.

In the course of the year 1744, we had a great number of gouty rheumatisms, with fevers. The patients were deprived of the use of their limbs; the miliary eruption often came on, and seemed to relieve them, by restoring their limbs. In some their pains went off by forming phlegmons and erysipelas upon the extremities; some of which seized the arm and fore-arm, and were considerable enough to bring on the death of the patients: others were attended with large gangrenous eschars, which likewise frequently proved fatal.

Of all the remedies, that did service in these disorders, decoctions of the bark, and the sudorific woods, as likewise that of scorfonera, were most effectual. But if a plentiful miliary eruption came on, notwithstanding the relief it seemed at first to procure, the event seldom turned out well.

The years 1745, 46, and 47, proved tolerably healthy: some disorders of the throat, becoming more common about the end of the last of these three years, were the fore-runners of the gangrenous sore throats of 1748, of which I have already made mention. In these cruel distempers the throat was in the same state with that of the larger intestines in 1743.



Great and frequent bleedings made the patients go off the sooner.

There were also this year malignant fevers, that began with rigors, fix'd pain in the head, pain about the heart, the fever in appearance very small, yet attended with delirium, and often with a miliary eruption.

Those, who died, had the villous coat of the stomach spread over with inflammatory spots, which swelled its substance, and gave it a brownish purple-colour. These spots were in greatest number about the upper orifice of the stomach.

The small guts had also some of these spots. I got drawings to be made of them.

Sometimes the glands of the mesentery were found obstructed, when the larger intestines, and other viscera, were in a sound state.

I cured, or rather stopp'd the progress of, these distempers, by giving, upon the first coming on of the rigors, a cordial and febrifuge electuary \*.

When the distemper did not yield to this remedy, we had recourse to small bleedings, and gentle physic. Such as were seized naturally with a slight flux, got well with the help of diluting liquors, made a little deterfive, such as limonade; but some of them lasted forty days, and above. The years 1749, 50, and 51, had the like malignant fevers, some of which were accompanied with violent colics in the begin-

\* Kinkina, one ounce: Venice-treacle and rhubarb, of each half an ounce: salt of centaury and wormwood, of each one drachm: syr. of mercurialis, q. s.

ing, followed with fluxes, which it was found necessary to moderate. I succeeded with one or two bleedings, after which I gave the decoctum album\*.

Some of these diseases had the appearance, at first, of a slight peripneumony, or cold, with perpetual faint sweats: then followed a drowsiness and stupor, a rambling for some moments at night, the belly puffed up, and uneasy, little or no urine, then a miliary eruption and delirium; and the patient was carried off in a few days.

The stomach in these subjects was inflamed, as also the small guts, by patches. In some there were ulcers, which almost penetrated the substance of the intestines.

Their lungs were full of blood, and in the back part, adhering to the pleura.

Those, who had a slight looseness only in the morning, which did not check the sweats, recover'd. Some of the malignant fevers, which we had at the Hôtel Dieu in 1750, were reported to be caused by infection conveyed in bales of horse-hair, to which was left some of the animals flesh, that was become putrified: and yet these fevers did not differ from others which we have already described.

Martha Renon, a girl of about twenty years of age, who died of this fever, had the mesentery filled with obstructed glands, and the intestines mortified in different places.

\* Crum of bread, two ounces; hartshorn-shavings, half an ounce; root of the greater comfrey, cut in slices, one ounce; to be boiled in a quart of water for a quarter of an hour: strain, and add an ounce of diacodium.

Francis Gentil had, besides these symptoms, almost the whole mesentery mortified, and an anthrax or carbuncle at the upper and fore-part of the arm-pit, and the whole body of a livid colour. This carbuncle proves, that these malignant fevers were something pestilential.

I say nothing of the small-pox, which hardly ever leaves this climate in any season of the year, but which nevertheless is more common towards the end of summer, and in autumn, and for the most part is accompanied with the miliary eruptions, which we have already observed to be joined to all these diseases, and which seldom failed to render them mortal.

I opened several of these variolous bodies, and in the greater number found superficial ulcers upon the nervous coat of the stomach, towards its upper orifice, with livid and inflammatory spots upon the other parts of the same, as also on the intestines (though in a small number), and the glands of the mesentery enlarged, and hardened.

In the year 1752, and beginning of 53, these malignant fevers, that put on the appearance of peripneumonies, became mortal in seven days, and we discovered, that they were occasioned by a suppurative inflammation of the pericardium. Laxative medicines, quickened by an emetic, were most successful against these inflammations.

About the end of the year 1753, and beginning of 54, these malignant fevers, which had their seat in the stomach, small guts, and partly in the lungs, appeared again, and seized a great number of persons of distinction. This circumstance made them be

looked upon as a new distemper by those, who did not attend to it sooner ; and the havoc they had usually made, being rendered more remarkable by the quality of those, who were the unhappy victims, gave us the reputation throughout Europe of having the plague.

These reasons redoubled the diligence of the gentlemen of the faculty. Our physicians met together, at their college, several times, to communicate their observations upon these diseases. I think they may be divided into three degrees.

The patients of the first degree felt, at the beginning, a lassitude, and pain in the joints, attended with some fevers, the fits whereof went off by sweats. They perfectly resembled those malignant, wandering, gouty rheumatisms of 1744. But these symptoms were of no long duration ; they left the patients long intervals, in which they were able to rise out of bed. There was no great danger attending ; and all that was terrible in them was this, that they were of long continuance.

The disease of the second degree had, besides the foregoing symptoms, a continual fever, with exacerbations, and a pain in the head, that increased as the fever increased.

That of the third degree began with the symptoms of the first, for four, five, and sometimes eight days : after which it passed to those of the second, and was besides accompanied, in the exacerbations, with a cough, sore throat, nausea, a dry, black, and foul tongue ; a delirium, or a tendency thereto, in the height of the fits, followed by sweats ; a remarkable stupidity in the remissions ; in some a

small oppreffion of the breast, with fputting of blood; in others a fwell'd belly, which was flow in every evacuation, efpecially that of urine. Afterwards there often appeared the miliary eruption: fome had a fmall flux, and blood was perceived in the ftools. In a word, one might remark all the fymptoms which we had in 1749, 50, and 51.

A great number were affected with a dejection of fpirits, and were ftruck with a fort of terror, as made them tremble at the found of a common voice.

Thefe difeafes ran through a courfe of thirty or forty days, which I think may be divided into four periods.

The firft, or firft feven days, were paffed with the fymptoms of the firft degree: the next feven days with thofe of the fecond degree. In the third period, which confifted of about the fame number of days, the patient laboured under all the fymptoms of the complete difeafe. Towards the twenty-firft the miliary eruption came on, which led the patient either to death on the twenty-fifth, or to recovery about the thirtieth or fortieth day.

Some patients, who were attacked with more violence, ran through all the ftages in feven days, as was remarked in 1752; and this fhort fpace brought fome perfons of the moft vigorous conftitutions to their graves.

Many of their bodies were opened, on which we made the following obfervations:

In fome, part of the villous coat of the ftomach, and of the fmall guts, was inflamed; and the reft of thefe organs were filled with an eruption of the miliary cryftalline kind, except that it was larger:  
and

and there was likewise an obstruction in the glands of the mesentery.

In others, a strong inflammation had seized the whole stomach, and a small portion of the oesophagus; but the intestines were free. These were filled with wind in those subjects, whose bellies had been swelled.

In those cases, where the delirium had continued long and violent, we found either ulceration on the stomach, or its villous coat separated, together with a great inflammation, and even some gangrenous spots on the other coats of that organ. Nothing extraordinary was ever found in the brain.

The most successful method of treating these disorders, was as follows :

A bleeding or two, at first, was directly followed by a \* vomit. I have seen this remedy produce a small flux of five or six stools a day, which, with the addition of limonade, was generally sufficient to effect a cure. But when this success did not follow, the patient was bled first in the arm, then in the foot, and every two or three days there was given some cassia, quickened by an emetic, and dissolv'd in a decoction of tamarinds. We prescribed ptisans of strawberry-leaves, adding some nitre; limonades,

\* It is called in the original, *l'emetique en lavage*, which signifies an emetic well diluted with water; the formula of which is, four grains of emetic tartar, dissolved in a quart of water; the fourth part of which is given at a time. After this has work'd either by vomit or stool, another fourth is taken, and so on, till the patient is supposed to have vomited or purged enough.

clarified whey, pure water by itself, a good many simple clysters; draughts of the distilled water of borage and buglofs, sweeten'd with fyrup of lemons and water-lily.

Many did well with a simple julep of fugar and water, and a little wine.

There were some, who, when they were just sinking, were raised again by cordials of the warmest kind, such as Venice-treacle, given in large doses; and the preparation, called vinegar of the four thieves \*, by spoonfuls, in broth. These medicines brought out a most plentiful miliary eruption, by which they were cured.

The manner of recovery from this disease deserves a place in the history of it. There were but few, who recovered of it in the usual way, that is to say, who only wanted the restoration of their strength, exhausted as well by sickness as the medicines. Almost all of them, even those who had it in the first

\* This is an infusion of several aromatic plants in vinegar. The reason of its being called *vinaigre des quatre voleurs*, is this:

When the plague raged at Marseilles, four rogues broke into the houses of the sick, and carried off what they pleased, retiring to a secret place with their booty, and returned to the same business at different times, till they had amassed great riches; but were at last apprehended, and hang'd. Being ask'd, how they durst venture into the pestilential houses? they said, they preserved themselves by drinking a glass of their vinegar twice or thrice a day, sprinkling their handkerchiefs and clothes with the same, and were not afraid. The French retain this name for it, though it is not in their dispensatories, and use it as a high cordial.

and

and second degree, still felt some remains of the symptoms of the disease. Such patients, as had any critical abscesses, were saved by this tribute only; but others, who escaped the mortality of this dangerous poison, carried about with them for several months, and still feel, its terrible effects: for to the usual weakness of convalescents were joined palpitations of the heart; a little of the painful lassitude in the joints, which was a sign of the first attack of the disease; a slight pain in the head, but almost constant; an uncertain pulse; and, upon the lessening or cessation of these complaints, they were replaced by wandering pains in the hypochondrias, swimings in the head, melancholy, and a remarkable disposition to fear, being the remains of what constituted one of the characteristics of the disease.

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XVI. *An Account of the Death of Mr. George William Richman, Professor of experimental Philosophy, a Member of the Imperial Academy of Sciences at Petersburg. Translated from the High-Dutch.*

Read March 13, 1755. **N**obody can be ignorant, who has perused the works of the Academy at Petersburg, or even only the public papers, with what application the late Professor Richman had studied, among other branches of philosophy, that of the electricity of bodies in general, and that of thunder-



der-clouds in particular. The discoveries of the demonstrators of electricity, by which they are enabled, not only to determine its degrees of electrical force, which is produced by the rotation and friction of a glass ball, but likewise those, that are emitted by thunder-clouds, are peculiar to him. And it must be said, to his honour, that he has made as many discoveries on this subject, as, I will venture to say, any naturalist has; and of which the learned world will be made more sensible, by reading his treatise, when it comes from the press, which he intended to have read, the 6th of September, this year 1753, at a publick meeting of the members of the academy. In order therefore to demonstrate what he might advance in this discourse with the greater certainty, he neglected no opportunity, upon the appearance of a thunder-cloud, diligently to discover its strength. Bars were standing for this purpose always upon the roof of the house. These received the electrical power of the clouds, and imparted it to certain chains fastened to them; by which it was conducted into one of his rooms, where his apparatus was. He was attending the usual meeting of the Academy the 26th of July 1753, a little before noon, when it thundered at a pretty distance, the sky being clear, and the sun shining. Upon this he hastened home, in hopes of confirming his former observations, or possibly enabling himself to make new ones. The engraver Sokolow, who had the care of his future treatise, accompanied him, to make himself the better acquainted with the chief circumstances of the electrical experiment, in order to be thereby enabled to represent it more justly on a copper-

copper-plate. Mr. Richman carried the engraver immediately to his apparatus, taking notice of the degree of electricity on his bar, which was then only four; and by which it appeared, that his bar had received very little from the thunder. He described to Mr. Sokolow the dangerous consequences, which would attend the electrical power being magnified to the 45th, or more degrees of his expositor. In the mean time the misfortune happened, about half an hour after noon, which cost Professor Richman his life. A thick cloud, that came from the north-east, and seemed to float very low in the air, was taken notice of by people walking the street; and these affirm, that they could plainly see, upon the subsequent flash of lightning, and peal of thunder, a quantity of vaporous matter issue from it, which diffused itself in the circumjacent space. It was such a thunder-clap, as has hardly been remembered at Petersburg. The serene weather continued afterwards just as before. In those places, which had not been obscured by the thunder-cloud, the sun was as powerful as ever, and a very little shower of rain only followed the clap. An English captain observed, that as the wind had been till then easterly, it, not long before the thunder, veered about to westward, but immediately after the stroke it returned to its former point, east. By this it appears, in what manner the inflammation of the electrical particles followed so quickly, the wind driving it against another cloud, not so pregnant with that combustible matter. The neighbours declare, that they saw through their windows a vapour, in different rays, dart along the whole extent of the street; and that  
where-

where-ever it touched the ground, it emitted everywhere sparks; which is not incredible; for besides, there were people, who, walking along between these rays of vapour, were quite stunned, and some beat to the ground, but they speedily recovered themselves.

A centinel in the Great Perspective \*, not far from Mr. Richman's dwelling, which stands at the corner of the said Perspective, was cast some paces from his centry-box, but without receiving any injury. It is not therefore to be doubted, but that this very thunder-cloud, or its electrical discharge, must have struck the iron bars, which were upon Mr. Richman's house-top; by which a great part of the electric force was conducted, by means of the chains, to his electrical expofitor; and thus it could not fail of having the melancholy effect, the parallel of which has not been known. According to the relation of the engraver Sokolow, Mr. Richman inclined his head towards the expofitor, to observe what degree of force it would have; and whilst he stood in that bent posture, a great white and bluish fire appeared between the electrical expofitor and Mr. Richman's head. At the same time arose a sort of stream, or vapour, which intirely numbed the engraver, and made him sink down upon the ground; so that he cannot remember to have heard the loud thunder-clap. The iron ruler belonging to the expofitor, which hung perpendicular, as it received all the force from the bars and chains, cast from it a thread, which was fix'd to its top, and drove it upwards towards the expofitor. That this ruler might point out the de-

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\* Probably a street so called.

grees of strength, I take it, that for its more powerful operation, it stood with its lower end in a glass vessel, which was filled with brass filings. This ruler hanging right, a globular flame hath been always produced, as well by artificial electricity as that of the clouds, which may be denominated natural electricity. This being now stopped, by the filings and glass vessel, from taking its direction downwards, seems to have expanded itself round-about the ruler, and by those bodies, incapable of electricity, to have been carried on towards Mr. Richman. And this is further confirmed, because they afterwards found the vessel broken in pieces, and the filings scattered about. The particulars, which happened to Mr. Richman, Mr. Sokolow is ignorant of. As soon as he had recovered his senses, he got up, and ran out of the house, acquainting every one, whom he met in the street, that the thunder had struck into Mr. Richman's house. On the other side, as soon as Mrs. Richman heard the very loud stroke of thunder, she came hastening into the chamber, in which she conjectured she should see the bad consequences. She found her husband past sensation, sitting upon a chest, which happened to be placed behind him, and leaning against the wall; which situation must have been occasioned by his falling back upon receiving the electrical blow. He was no sooner struck, but killed. There were not the least appearances of life. A sulphureous smell, not unlike that which is caused by the explosion of gun-powder, diffused itself through the whole house. Some servants, who were hard by in the kitchen, felt its effects, since they were thereby quite stupified. The electrical expositor stood upon a low

beaufet, upon which was likewise placed a china bowl, that was crack'd : and there was moreover such a shaking in the house, that the shock even stopp'd the movement of an English clock, or pendulum, which was in an adjoining room. At first it was not known whether the windows of Mr. Richman's chamber had been shut, or open ; but it is recollected, that, in preparing the apparatus, it is never opened : it would be moreover impracticable, lest the thread of the expositor should receive any motion from the wind or air, which would rush in. There was no other inflammation happened in the house. But we have found another effect of the force of electricity, or of thunder-bolts, discoverable by the door-posts of the house ; for they were rent asunder length-ways, and the door, together with that part of the posts, so torn away, twirled into the porch. The reason of which appears to be, because one of the above-mentioned chains, that were carried from the bars at the house-top to the expositor, passed very near them : and the kitchen-door, being at a little distance off, had a splinter torn out, and dashed against a stair-case, that went towards the top of the house ; so that part of the electrical matter seems to have taken its course this way, but without doing any more damage. All the neighbourhood, I may indeed say the whole island, was immediately in an uproar, partly by the relation of the engraver and the centinels, and partly by the servants being sent directly to the friends and acquaintance of the deceased. There never was a report of a misfortune so speedily spread abroad in this city, as this was : every one ran thither, altho' they

they had connexion with, nor any manner of acquaintance in the house. The upper Maitre de Police was presently there, and placed a guard, to prevent the concourse of the populace, which was relieved by another, the same day, sent from the Academy. They opened a vein of the breathless body twice, but no blood followed. They endeavoured to recover sensation by violent chafing, but in vain. Upon turning the corpse topsy-turvy, during the rubbing, an inconsiderable quantity of blood fell out of the mouth. There appeared a red spot on the forehead, from which spirted some drops of blood through the pores, without wounding the skin. The shoe belonging to the left foot was burst open. Uncovering the foot at that place, they found a blue mark, by which it is concluded, that the electrical force of the thunder having forced into the head, made its way out again at the foot. Upon the body, particularly on the left side, were several red and blue spots, resembling leather, shrunk by being burnt. Many more blue spots were afterwards visible over the whole body, and in particular on the back. That upon the forehead changed to a brownish-red. The hair of his head was not singed, notwithstanding the spot touched some of it. As for the wig, the deceased had taken it off. In the place, where the shoe was unript, the stocking was intire; as was his coat every-where, the waistcoat being only singed on the fore-flap, where it joined the hinder. But there appeared on the back of the engraver's coat long narrow streaks, as if red-hot wires had burnt off the nap. We conjecture it was occasioned by the thread hanging in the chamber, by the chains of the expo-

fitor ; and that some of it might fall upon him ; but we do not assert it positively ; for we are more certain of this, that the people in the house, who first of all went thither, took down the chains, thread, and other things belonging to the apparatus, immediately after. When the body was opened the next day, twenty-four hours afterwards, the cranium was very intire, having no fissure or cross-opening ; the brain as found as possibly it could ; the transparent pellicles of the wind-wipe were excessively tender, gave way, and rent easily. There was some extravasated blood in it, as likewise in the cavities below the lungs ; those by the breast being quite sound, and and not damaged, but those towards the back of a brownish black colour, and filled with more of the above blood ; otherwise none of the entrails were touched : the throat, glands, and the thin intestines, were all inflamed. The singed leather-colour'd spots penetrated the skin only. In short, although one could trace out all the consequences of an instantaneous stroke throughout the whole body, yet many of them have not appeared to happen to others struck by thunder, when they have been examined. Should not one therefore be led to conclude, that the electrical force, that occasioned Mr. Richman's death, must have been of a different substance than the common thunder-bolt ? That it was much more subtle, is obvious, because it left so few visible traces in the body, which it penetrated. Twice twenty-four hours being elapsed, the body was so far corrupted, that it was with difficulty they got it in a coffin. He was buried the 29th, followed by a considerable train of mourners. Those, who had the  
pleasure

pleasure to be more intimately acquainted with him, are in doubt, which they should give the preference to, his knowledge and assiduity, or his piety and sincerity, and his good qualities in general; and which is most to be lamented, the loss, which the Academy has sustained, or that, which his family must suffer. He was born the 11th of July, 1711, at Pernaú, after the decease of his father, Mr. William Richman, treasurer of the king of Sweden, who was carried off by the plague, at the close of the year 1710. Having laid the foundation of his learning at the Gymnasium at Revel, he prosecuted his studies at the Universities of Halle and Iena, where he always made the mathematics and philosophy the principal objects of them. He was made a member of the Imperial Academy in the year 1735; extraordinary Professor in 1741; and at last, in 1745, ordinary Professor of experimental philosophy. In his later years he married his present disconsolate widow, by whom he has had six children, three of whom died before him, but two sons and a daughter survive him.

XVII. *An Account of a Roman Inscription found at Malton in Yorkshire, in the Year 1753. By John Ward, LL. D. Rhet. Prof. Gresh. and V. P. R. S.*

Read March 20, 1755. **T**HIS inscription was dug up in the Pye Pits, over against the lodge at Malton, a town situated on the river Derwent, in the



the North Riding of Yorkshire. Soon after its discovery, which was in the beginning of the year 1753, a copy of it, with the draught of the stone, was sent by the Reverend Mr. James Borwick, Minister of Whitby, in that county, to Mr. Francis Drake of York, a worthy member of this Society. But that copy not being accurately taken, Mr. Drake procured a more exact one from Mr. Percival Luccock of Malton, the present possessor of the stone. Both those copies were transmitted to me by Mr. Drake; the latter of which having been taken by laying a paper over the inscription, and tracing out the letters upon it, exhibits both their true size and form. That copy, with another drawn from it, but reduced by a scale of one fourth of the original, accompany this paper (1).

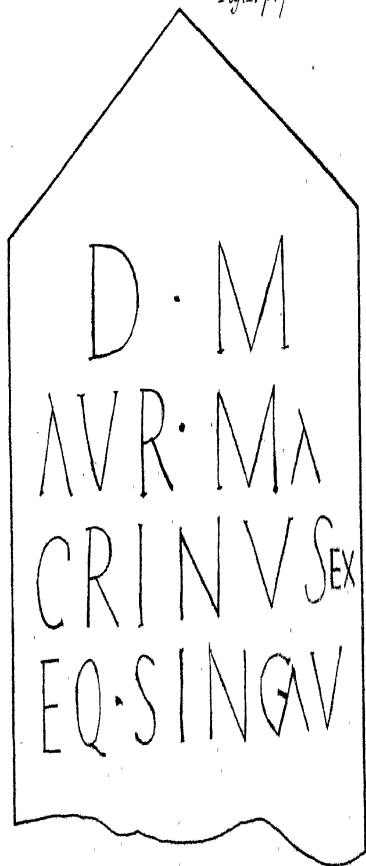
The shape of the stone, as delineated in the draught of Mr. Borwick, is given in the lesser of these copies, which shews it to have been broken off at the bottom. But Mr. Drake informs me, that nothing more was cut upon the stone, as may be concluded from the distance of the fracture below the writing; for had the inscription been continued farther, part of the letters at least of the following line would have appeared at the bottom. It is true indeed, that epitaphs written in this form usually end with the name of the person, who erected the monument; one example of which I shall produce from Montfaucon, as it relates to a soldier of the same character.

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(1) The reduced draught, which has been engraved, may be seen in *Plate III. Fig. 1.* of this Volume.



*Fig. 1. p. 71.*

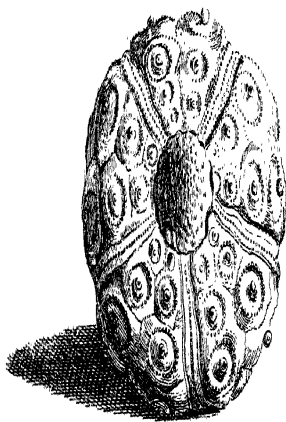


*Fig. 2.*

*p. 156.*



*Fig. 3*







D M

T· AVREL SVMMVS EQ  
 SING· AVG· CLAUDIO  
 VIRVNO NAT· NORIC (1)  
 VIXIT ANN· XXVII MIL  
 ANN· VIII· P· AELIVS  
 SEVERVS HERES  
 AMICO OPTIMO F (2)

But notwithstanding this appears to have been the usual method of composing such inscriptions, yet it was not always observed; and we meet with some few instances expressed with the like brevity, as that under consideration, which in words at length may be read in the following manner :

*Diis Manibus. Aurelius Macrinus, ex equitibus singularibus Augusti.*

The peculiarity of this inscription, and what renders it remarkable, is the character of the person, to whose memory it was erected. These *equites singulares* are often mentioned in Gruter, Fabretti, and other collectors of antient monuments; but this is the first instance of them, which has ever occurred in any of our British inscriptions. Modern writers have differed very much in their sentiments, concerning the particular office and duty of this part of the Roman cavalry; but I shall content myself with mentioning only, what appears to me most probable.

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(1) The fourth line of this inscription may, I think, be read thus: *Viruno oriundus, natione Noricus.*

(2) *Diar. Ital. pag. 115.*

It is plain from Hyginus, who lived under the reigns of Trajan and Hadrian, that these *equites singulares* made part of the emperor's body guards. For in describing the disposition of a Roman camp he says: *Equites praetoriani locum accipiunt latere dextro praetorii, singulares imperatoris latere sinistro; quorum si major numerus fuerit, utpote singulares DC, praetoriani CCC, poterunt CL singulares in striga praetorianorum tendere* (1). They are here called *equites singulares imperatoris*, as also in some inscriptions; but others for *imperatoris* have *Augusti*, as ours; and others again *Caesaris*, or *Domini nostri*; but that transcribed above from Montfaucon has *Augusto Claudio*; and some few only the general title of *equites singulares* (2). Reinesius therefore was of the opinion, that they not only attended the emperors themselves, but also the governors of the Roman provinces, in the like station (3); tho Fabretti, who has given us a large collection of these inscriptions, declares, that he had met with no sufficient evidence of this, either from antient writers or inscriptions (4). Schelius, in his notes upon this passage of Hyginus (5), thinks, that they were first instituted by Augustus; and that Tacitus refers to them, when he says: *Accessit ala singularium excita olim a Vitellio, deinde in partes Vespasiani transgressa* (6). And there is an

(1) *Hygin. gromat. p. 4. col. 1. vers. 7.* as the text is afterwards corrected by R. Herm. Schelius, edit. *Amstel.* 1660.

(2) *Gruter. passim.*

(3) *Syntagm. inscript. antiq. class. 1. num. xvi. pag. 41.*

(4) *Inscript. antiq. pag. 357.*

(5) *Pag. 44.*

(6) *Hist. lib. iv. cap. 70.*

inscription in Gruter, which mentions one of these *equites singulares*, as having served under Augustus in several of his wars, and been rewarded by him (1).

This account of the origin and station of that body of Roman horse may afford some light in settling the time, when this funeral monument of *Aurelius Macrinus* was erected. For if they always attended on the emperor himself, some one of the Roman emperors must then have been resident in Britain. And as there appears no probable reason for assigning it to any of them before Hadrian; so there are some circumstances relating to the inscription, as will be shewn afterwards, which do not suit with his time. And after him there was no other emperor in Britain, before the reign of Severus. Indeed Albinus, who then governed here, had been complimented by him with the title of Augustus, before he came over himself. But he was soon after defeated and slain by him in Gaul, and we have no other inscription hitherto discovered in Britain, which has any relation to Albinus; whereas we have several, in which the name of Severus is expressly mentioned (2). As Severus therefore resided here for about three of his last years, and died at York; it seems to me most probable, that this monument was set up within that time. And to this both the form of the letters on the inscription very well agree, and the ligature of the two letters *G* and *A* at the end of it. For tho

(1) *Pag.* ccclxxi. *num.* 4.

(2) *Camden. Britann.* pag. 568. *edit.* 1607. *Ibid.* pag. 592. *Horsley, Brit. Rom. Northumb.* cix.



such combinations of letters in the same word were long before not uncommon among the Romans, both on their coins and monuments; yet I meet with but one instance of it, where the two letters belong to different words, as they do here, before the time of Severus; and that was in the reign of Commodus, who was soon succeeded by him. That monument was erected in the consulship of Apronianus and Bradua, whose names are expressed in the inscription (1). But in after times such ligatures were more common, so that we meet with three or more letters sometimes combined in that manner (2).

Fabretti observes, that these *equites singulares* had a burying place allotted them at Rome, in the *Via Labicana*, not far from the sepulchre of the empress Helena. Several of their monuments have been found in that coemetry, adorned at the top with an human figure, lying on a couch; and below the inscription, a horse with trappings, and a boy holding a whip. And if any such are met with elsewhere, they have, as he supposes, been removed from thence (3). Montfaucon has given us a draught of one of those monuments, which contains the inscription recited above (4), and answers to this description of Fabretti, both as to the human figure, and that of the horse; the former of which has a *patera* in the left hand, and a mask is suspended at each end of the couch; and the boy, who is there

(1) *Horsley, Britann. Rom. Cumberl. lvii.*

(2) See *Britann. Rom.* in the *Table of ligatures*, pag. 189.

(3) *Uli supra*, pag. 360.

(4) *Pag. 71.*

wanting, he found upon another (1). Those ornaments might very probably be omitted on such monuments, when erected in the provinces; and it is plain, there could not be room for the human figure above the inscription in this of Malton. At which place, as Mr. Borwick says in his letter, many urns, coins, and other remains of antiquity have been found, in and about the Pye Pits; from whence he supposes it to have been a coemetry for some Roman garrison.

P. S. Among the inscriptions in Gruter, relating to the *equites singulares*, there is one of them, where this title is used in a very different sense, from that given of it in the above discourse; for which reason I shall here transcribe it.

MARTI. CAMPESTRI. SAC  
 PRO. SAL  
 IMP. M. AVREL. COMMODI  
 AVG. ET. EQVIT. SING  
 T. AVREL. DECIMVS  
 7. LEG. VII. G. FEL  
 PRAEP. SIMVL. ET  
 CAMP. DEDIC. K. MART  
 MAMERT. ET. RVFO. COS (2).

In this inscription the emperor Commodus is himself called *eques singularis*, for the explication of which character recourse must be had to the accounts

(1) *Diar. Ital.* pag. 115, 117.

(2) *Pag.* lvii. *num.* 12.

given by historians of his life and actions. And among other instances of his base and infamous conduct, he is said to have demeaned himself to that degree, as to act a part in most of the public games, that were celebrated at Rome. Thus one of his diversions was to attack wild animals in the amphitheatre; at which exercise he was so expert, as never to miss his aim in killing them, either with a javelin or an arrow (1). He would often combat with the gladiators, and was so fond of that character, that he assumed the name of one of them, who had been very famous (2). At other times he would act as a charioteer in the *Circus* (3). He joined also in the athletic exercises, and was at last strangled by a champion, with whom he had formerly engaged (4). I do not find indeed, that he is ever mentioned by historians as a racer on a single horse, which is the character given him in the inscription; as appears from Isidore, who calls them *equites singulares*, as distinguished from the *desultores* (5). But that horse racing was also one of his recreations, we learn from a passage in Dion Cassius; who says, that Commodus came once to Rome on a fuddain, when he was not expected, and exhibited a race of thirty horses in the space of two hours (6). It is not improbable therefore, that he might sometimes take a

(1) *Herodian. in vit. cap. 15.*

(2) *Ibid. Lamprid. in vit. p. 50. edit. Paris. 1620.*

(3) *Lamprid. pag. 47.*

(4) *Id. pag. 52.*

(5) *Orig. Lib. xviii. cap. 35.*

(6) *In vit. pag. 825. edit. Leunclæv.*

part in that exercise, as well as in those above mentioned. And as he affected to have all his actions, however shameful or ridiculous, publicly recorded (1), this inscription might have been erected in compliment to him under that character. Mr. Hearne, who has published this inscription, seems to think, that Commodus might there be stiled *equus singularis*, like the Grecian and Trojan heroes in their single combats, who are described by Homer, as ἵπποσύνη τε καὶ ἀνδρείῳ πεποιδότες (2). But the inscription is dedicated MARTI CAMPESTRI, which is a title given to that deity, not as a warrior, but as presiding over the public games in the *Campus Martius* at Rome. And agreeably to this sense of the word Horace saies,

*Ludere qui nescit, campestribus abstinet armis* (3).

J. W.

(1) *Lamprid. pag. 50, 51.*

(2) *Praef. ad Guil. Neuburg. Hist. rer. Angl. pag. liii.*

(3) *A. P. v. 379.*

XVIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the worshipful Company of Apothecaries, for the Year 1754, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. Societat. Pharmacent. Lond. Socius, Hort. Chelf. Præfectus & Prælector Botan.*

- Read April 10, 1601 **A** Ntirrhinum Siculum Linariæ  
1755. folio, niveo flore, Tilli.
- 1602 Calcalia caule herbaceo foliis hastato-sagittatis  
denticulatis, petiolis superne dilatatis. Hort.  
Upfal. 254.
- 1603 Calamintha Hispanica frutescens Mari folio.  
Inst. R. H. 194.  
Melissa fruticosa ramis attenuatis virgatis foliis  
subtus tomentosis. Linn. Sp. Plant. 593.
- 1604 Calendula foliis obverse ovatis denticulatis,  
caule fruticoso perenni. Fl. Leyd. Prod. 531.
- 1605 Campanula hortensis rapunculi radice repente.  
M. H. 2. 460.  
Campanula repens flore minore cæruleo. J. B.  
2. 706.
- 1606 Caryophyllus minimus muralis. C. B. 211.  
Lychnis minima muralis. M. H. 2. 547.
- 1607 Cephalanthus foliis oppositis ternisque. Fl.  
Virg. 15.

Scabiosa dendroides Americana ternis foliis  
caulem ambientibus, floribus ochro-leucis.  
Pluk. Alm. 336.

1608 Cerinthe quorundam major, fl. ex rubro-pur-  
purascente. J. B. 3. 603.

1609 Chenopodium Lini folio villoso. T. 506.  
Linaria scoparia. C. B. P. 212.

1610 Clematis Hispanica surrecta altera et humilior,  
fl. albicante. H. R. Par.

1611 Clymenum Hispanicum flore vario, filiqua ar-  
ticulata. T. 396.

Lathyrus vicioides vexillo rubro, petalis ro-  
strum ambientibus cæruleis. M. H. 2. 50.

1612 Cochlearia altissima Glaſti folio. T. 215.  
Lepidium. C. B. 96.

1613 Conyza latifolia viscosa suaveolens flore aureo,  
ex Gallo-provincia. Tourn. 455.

Conyza Pyrenaica foliis Primulæ veris. Par.  
Bat. 127.

1614 Coreopsis foliis linearibus integerrimis caule  
erecto. Fl. Virg. 18.

1615 Corispermum Hyssopifolium. D. Jussieu, Ac.  
Reg. Sc. 1712.

1616 Digitalis Hispanica purpurea minor. T. 165.  
Digitalis Verbaſci folio purpurea minor pe-  
rennis Hispanica. Bar. Icon. 1183.

1617 Dipſacus folio laciniato. C. B. P. 385.

1618 Dodartia foliis lanceolatis ferratis, floribus spi-  
catis. Linaria Bellidis folio. C. B. P. 212.  
Prod. 106.

1619 Fabago triphylla et tetraphylla flore tetrape-  
talo, fructu membranaceo quadrangulati.  
Burm. pl. Afric. 7. Tab. 3.

- Zigophyllum capsularum* angulis compresso membranaceis. Linn. Sp. Plant. 383.
- 1620 *Geranium Anemones folio*. Pal. Bat 178.
- 1621 *Heliotropium Canariense* arborefcens, fol. Scorodoniæ. H. Am. 129.
- 1622 *Hermannia* fol. Ibisci hirsuto, flore primo luteo, deinde purpuro-rubello. An *Hermannia frutescens* folio oblongo molli cordato hirsuto. Boerh. Ind. Alt.
- 1623 *Hesperis lutea* filiquis strictissimis. Tourn 222. *Draba lutea* quibusdam. J. B. 2. 870.
- 1624 *Hieracium Echioides* capitulis Cardui benedicti majus Arabicum folio sinuato. Hort. Edinburg.
- 1625 *Hieracium Pyrenaicum* longifolium amplexicaule. T. 472.
- 1626 *Jasminoides aculeatum* humile Halimi minoris folio, flore majore violaceo, fructu crassiore per maturitatem flavescente. Michel. Nov. Gen. 224.
- 1627 *Ketmia Ægyptiaca* Vitis folio, parvo flore. Tourn.
- 1628 *Lathyrus Tingitanus* filiquis Orobi, fl. amplo ruberrimo. M. H. 2. 55.
- 1629 *Lotus leguminibus conjugatis* membranaceo quadrangulis bracteis oblongo-ovatis. Linn. Sp. Plant. 774.  
*Lotus luteus* filiqua angulosa. Boer. Ind. Alt. 2. p. 37.
- 1630 *Lychnis segetum* rubra foliis Perfoliatae. C. B. 204. *Vaccaria*. Dod. Pempt. 104.
- 1631 *Marrubium Hispanicum* calyce stellato et aculeato. I. R. H. 182. *Alysson* Galeni. Clus. Hist. 35.

- 1632 *Medica* major erectior, floribus purpurascen-  
tibus. Tourn. 410.
- 1633 *Mespilus* foliis ovatis serratis, pomis ovatis ex  
luteo virentibus nobis.
- 1634 *Mespilus* foliis ovatis, obtusis, crenatis, spinis  
longis tenuibus, fructu singulari rotundo,  
umbilico foliaceo. An *Mespilus* Virginiana.  
Uvæ crispæ foliis. Pluk. Mant.
- 1635 *Ornithogalum* humile foliis ovatis sessilibus,  
floribus corymbosis, nobis.
- 1636 *Osteospermum* foliis oblongis acute dentatis,  
caule fruticoso diffuso, nobis.
- 1637 *Othonna* foliis pinnatifidis laciniis linearibus  
parallelis. H. Cliff. 419. Jacobæa Africana  
frutescens foliis Absinthii umbelliferi incanis.  
H. Amst. 2. p. 137.
- 1638 *Pelecinus* vulgaris. T. 417. *Astragalus* purpu-  
reus annuus peregrinus, siliquis utriusque  
ferræ similibus. M. Hist. 2. 107.
- 1639 *Phyllis* stipulis dentatis. Flor. Leyd. Prod. 92.  
*Valerianella* Canariens. frutescens simpla nobla  
dicta. Hort. Eltham 405.
- 1640 *Plantago* angustifolia paniculis Lagopi. C. B.  
374.
- 1641 *Ruyschiana* floribus verticillatis, nobis.
- 1642 *Samolus* Valerandi. J. B. 3. 791. *Anagallis*  
aquatica folio rotundo non crenato. C. B. P.  
252.
- 1643 *Scabiosa* stellata folio laciniato major. Tourn.
- 1644 *Solanum* spiniferum frutescens spinis igneis  
Americanum. Pluk. Phyt. 225. fig. 5.
- 1645 *Telephium* Dioscoridis Imperati.



- Telephium repens folio non deciduo. C. B. P.  
287.
- 1646 Thalictrum minus alterum Parisiense foliis  
crassioribus et lucidis. H. R. Par.
- 1647 Tithymalus maritimus. C. B. P. 291.  
Tithimalus paralius. J. B. 3. 674.
- 1648 Trifolium Bitumen redolens. C. B. 327.
- 1649 Trifolium Bitumen redolens angustifolium.  
Boer. Ind. Alt. 2. p. 32.  
Trifolium bituminosum arboreum angustifo-  
lium ac sempervirens. Hort. Cath.
- 1650 Virga aurea Canadensis foliis carnosis non fer-  
ratis latioribus. Hist. Oxon.

XIX. *A Letter to the Right Honourable  
George Earl of Macclesfield, President of  
the Royal Society, on the Advantage of  
taking the Mean of a Number of Obser-  
vations, in practical Astronomy: By  
T. Simpson, F. R. S.*

My Lord,

Read April 10, 1755. **I**T is well known to your Lordship,  
that the method practised by astrono-  
mers, in order to diminish the errors arising from the  
imperfections of instruments, and of the organs of sense,  
by taking the Mean of several observations, has not  
been so generally received, but that some persons, of  
considerable note, have been of opinion, and even  
publickly maintained, that one single observation,  
taken

taken with due care, was as much to be relied on as the Mean of a great number.

As this appeared to me to be a matter of much importance, I had a strong inclination to try whether, by the application of mathematical principles, it might not receive some new light; from whence the utility and advantage of the method in practice might appear with a greater degree of evidence. In the prosecution of this design (the result of which I have now the honour to transmit to your Lordship) I have, indeed, been obliged to make use of an hypothesis, or to assume a series of numbers, to express the respective chances for the different errors to which any single observation is subject; which series, to me, seems not ill-adapted: but this I shall submit intirely to the judgment of your Lordship, who have made so great a number of observations, at your seat at Shirburn; where, to the best collection of mathematical books, your Lordship has added a more complete set of astronomical instruments than (perhaps) are to be found in the possession of any nobleman in Europe.

Should not the assumption, which I have made use of, appear to your Lordship so well chosen as some others might be, it will, however, be sufficient to answer the intended purpose: and your Lordship will find, on calculation, that, whatever series is assumed for the chances of the happening of the different errors, the result will turn out greatly in favour of the method now practised, by taking a mean value. But I shall no longer detain your Lordship with general observations, but proceed to the

matter proposed; which I shall consider in the following propositions.

PROPOSITION I.

Supposing that the several chances for the different errors that any single observation can admit of, are expressed by the terms of the progression  $r^{-v} \dots r^{-3}, r^{-2}, r^{-1}, r^0, r^1, r^2, r^3 \dots r^v$  (where the exponents denote the quantities and qualities of the particular errors, and the terms themselves the respective chances for their happening): 'tis proposed to determine the probability, or odds, that the error, by taking the Mean of a given number ( $n$ ) of observations, exceeds not a given quantity  $\left(\frac{m}{n}\right)$ .

It is evident, from the laws of chance, that, if the given series,  $r^{-v} \dots + r^{-3} + r^{-2} + r^{-1} + r^0 + r^1 + r^2 + r^3 \dots + r^v$ , expressing all the chances in one observation, be raised to the  $n^{\text{th}}$  power, the terms of the series thence arising will truly exhibit all the different chances in all the proposed ( $n$ ) observations. In order to raise this power, with the greatest facility, our given expression may be reduced to  $r^{-v} \times \frac{1 - r^{2v+1}}{1 - r}$  : whereof the  $n^{\text{th}}$  power (making

$w = 2v + 1$ ), will be  $r^{-nw} \times \overline{1 - r^w}^n \times \overline{1 - r}^{-n}$ ; which, expanded, becomes

$$r^{-nw} - nr^{w-nv} + \frac{n \cdot n-1}{1 \cdot 2} r^{2w-nv} - \frac{n \cdot n-1 \cdot n-2}{1 \cdot 2 \cdot 3} r^{3w-nv} + \&c.$$

multiplied into

$$1 + nr + \frac{n \cdot n+1}{1 \cdot 2} r^2 + \frac{n \cdot n+1 \cdot n+2}{1 \cdot 2 \cdot 3} r^3 + \&c.$$

Now,

Now, to find from hence the sum of all the chances whereby the excess of the positive errors above the negative ones can amount, precisely, to a given number  $m$ , it will be sufficient (instead of multiplying the former series by the whole of the latter) to multiply by such terms of the latter, only, as are necessary to the production of the given exponent  $m$ , in question. Thus, the first term ( $r^{-nv}$ ) of the former series is to be multiplied by that term of the second, whose exponent is  $nv + m$ , in order that the power of  $r$ , in the product, may be  $r^m$ . But it is plain, from the law of the series, that the coefficient of this term (putting  $nv + m = q$ ) will be  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q)$ ,  $q$  being the number of factors; and consequently, that the product under consideration will be  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q) \times r^m$ . Again, the second term of the former series being  $-nr^{w-nv}$ , the exponent of the corresponding term of the latter will be  $-w + nv + m (=q - w)$ , and therefore the term itself equal to  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-w) \times r^{q-w}$ : which, drawn into  $-nr^{w-nv}$ , gives  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-w) \times -nr^m$ , for the second term required.

In like manner, the third term, of the product, whose exponent is  $m$ , will be found  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-2w) \times \frac{n}{1} \cdot \frac{n-1}{2} r^m$ : and the sum of all the terms having the same given exponent ( $m$ ) will consequently be

$$\begin{aligned}
 & + \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q) \times r^m \\
 & - \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-w) \times n r^m \\
 & + \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-2w) \times \frac{n}{1} \cdot \frac{n-1}{2} ; \\
 & - \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-3w) \times \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} r^m \\
 & \quad \mathcal{E}c. \quad \mathcal{E}c.
 \end{aligned}$$

From which general expression, by expounding  $m$  by 0, +1, -1, +2, -2, &c. successively, the sum of all the chances, whereby the difference of the positive and negative errors can fall within the proposed limits, will be found; which, divided by  $r^{-m} \times \overline{1-r^w}^n \times \overline{1-r}^{-n}$ , will give the true measure of the probability required: from whence the advantage of taking the Mean of several observations might be shewn: but this I shall exemplify in the next proposition; which is better adapted to the purpose, and to which this is premised, as a Lemma.

*Remark.*

If  $r$  be taken = 1, or the chances for the errors in excess and defect be supposed exactly the same; then our expression, by expunging the powers of  $r$ , will become the very same with that shewing the chances for throwing  $n+q$  points with  $n$  dice; each die having as many faces ( $w$ ), as the result of any one single observation, can come out different ways. Which may be otherwise made to appear, independent of any kind of calculation, from the bare con-

sider-

consideration, that the chances for throwing precisely the number  $m$ , with  $n$  dice, whereof the faces, of each, are numbered  $-v, -3, -2, -1, 0, +1, +2, +3, \dots +v$ , must be the very same as the chances whereby the positive errors can exceed the negative ones by that precise number; which last are, evidently, the same as the chances for throwing precisely the number  $v+1. n+m$  (or  $n+q$ ) with the same  $n$  dice, when they are numbered in the common way, with the terms of the natural progression  $1, 2, 3, 4, 5, \&c.$ : because the number upon each face being, here, increased by  $v+1$ , the whole increase upon all the  $n$  faces will be expressed by  $v+1. n$ ; so that there will be now the very same chance for the number  $v+1. n+m$ , as there was before for the number  $m$ ; since the chances for throwing any faces assigned will continue the same, however those faces are numbered.

## PROPOSITION II.

Supposing the respective chances, for the different errors which any single observation can admit of, to be expressed by the terms of the series  $r^{-v} + 2r^{1-v} + 3r^{2-v} \dots + v+1. r^0 \dots + 3r^{v-2} + 2r^{v-1} + r^v$  (whereof the coefficients, from the middle one ( $v+1$ ), decrease, both ways, according to the terms of an arithmetical progression): 'tis proposed to determine the probability, or odds, that the error, by taking the Mean of a given number ( $t$ ) of observations, exceeds not a given quantity  $\left(\frac{m}{t}\right)$ .

Pursuing

Pursuing the method laid down in the preceding problem, the sum of the series here given will appear to be  $r^{-v} \times \frac{1 - r^{v+1}}{1 - r} \Big|^2$  (being the same

with the square of the geometrical progression  $r^{-\frac{1}{2}v} \times 1 + r + r^2 + r^3 \dots + r^v$ ), and the  $t^{\text{th}}$  power thereof, by making  $n = 2t$ , and  $w = v + 1$ , will therefore be given  $= r^{-tv} \times \frac{1 - r^{2w}}{1 - r} \Big|^n \times \frac{1 - r}{1 - r} =$

$$r^{-tv} - nr^{2w-tv} + \frac{n}{1} \cdot \frac{n-1}{2} r^{2w-tv} + \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} r^{3w-tv} + \&c.$$

multiplied into

$$1 + nr + \frac{n}{1} \cdot \frac{n+1}{2} r^2 + \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} r^3 + \&c.$$

Which series being the same with those in the preceding problem (excepting only that the exponents of the former of them are expressed in terms of  $t$ , instead of  $n$ ), it is plain, therefore, that if  $q$  be made  $= tv + m$  (instead of  $nv + m$ ), the conclusion, there brought out, will answer equally here: so that the sum of all the chances whereby the excess of the positive errors above the negative ones, can amount to a given number  $m$ , precisely, will be truly represented by

$$\begin{aligned} & + \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q) \times r^m \\ & - \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-w) \times nr^m \\ & + \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-2w) \times \frac{n}{1} \cdot \frac{n-1}{2} r^m \\ & - \frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} (q-3w) \times \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} r^m \\ & \&c. \qquad \qquad \qquad \&c. \end{aligned}$$

But

But this general expression, as several of the factors destroy each other, may be transformed to another, which is more commodious. Thus  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} \dots$

in the first line, will, by breaking the numerator & denominator into two parts, become

$$\frac{n \cdot \overline{n+1} \cdot \overline{n+2} \cdot \overline{n+3} \cdot \dots \cdot \overline{q \times q+1} \cdot \overline{q+2} \cdot \overline{q+3} \cdot \dots \cdot \overline{q+n}}{1 \cdot 2 \cdot 3 \cdot 4 \cdot \dots \cdot n \cdot \overline{n+1} \cdot \overline{n+2} \cdot \overline{n+3} \cdot \dots \cdot q}$$

which, by equal division, is reduced to

$$\frac{q \cdot \overline{q+1} \cdot \overline{q+2} \cdot \dots \cdot \overline{q+n-1}}{1 \cdot 2 \cdot 3 \cdot \dots \cdot n-1} = \frac{\overline{q+n-1} \cdot \overline{q+n-2} \cdot \dots \cdot \overline{q}}{1 \cdot 2 \cdot \dots \cdot n-1}$$

$$\frac{p-1}{1} \cdot \frac{p-2}{2} \cdot \frac{p-3}{3} (n-1); \text{ supposing } p (=q+n) = tv + m + n.$$

In the very same manner, making  $q' = q - w$ , and  $p' (=q' + n) = p - w$ , it appears, that  $\frac{n}{1} \cdot \frac{n+1}{2} \cdot \frac{n+2}{3} \dots$   
 $(q-w) = \frac{p'-1}{1} \cdot \frac{p'-2}{2} \cdot \frac{p'-3}{3} (n-1), \&c. \&c.$

and consequently, that our whole given expression (supposing  $p'' = p - 2w$ ,  $p''' = p - 3w$ , &c.) will be transformed to

$$\begin{aligned} & + \frac{p-1}{1} \cdot \frac{p-2}{2} \cdot \frac{p-3}{3} (n-1) \times r^m \\ & - \frac{p'-1}{1} \cdot \frac{p'-2}{2} \cdot \frac{p'-3}{3} (n-1) \times nr^m \\ & + \frac{p''-1}{1} \cdot \frac{p''-2}{2} \cdot \frac{p''-3}{3} (n-1) \times \frac{n}{1} \cdot \frac{n-1}{2} \cdot r^m \\ & - \frac{p'''-1}{1} \cdot \frac{p'''-2}{2} \cdot \frac{p'''-3}{3} (n-1) \times \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} r^m \\ & \qquad \qquad \&c. \qquad \qquad \&c. \end{aligned}$$

Which expression is to be continued till the factors become nothing, or negative; and which, when



$n=1$ , will be the very same with that exhibiting the number of chances for  $p$  points, precisely, with  $n$  dice, having each  $w$  faces: and in this case, where the chances for the errors in excess and defect are the same, the solution is the most simple it can be; since, from the chances above determined, answering to the number  $p$ , precisely, the sum of the chances for all the inferior numbers (inclusive) may be readily obtained; being given (from the Method of Increments) equal to

$$\begin{aligned}
 & + \frac{p}{1} \cdot \frac{p-1}{2} \cdot \frac{p-2}{3} \cdot \frac{p-3}{4} (n) \\
 & - \frac{p'}{1} \cdot \frac{p'-1}{2} \cdot \frac{p'-2}{3} \cdot \frac{p'-3}{4} (n) \times n \\
 & + \frac{p''}{1} \cdot \frac{p''-1}{2} \cdot \frac{p''-2}{3} \cdot \frac{p''-3}{4} (n) \times \frac{n \cdot n-1}{1 \cdot 2} \\
 & - \frac{p'''}{1} \cdot \frac{p'''-1}{2} \cdot \frac{p'''-2}{3} \cdot \frac{p'''-3}{4} (n) \times \frac{n \cdot n-1 \cdot n-2}{1 \cdot 2 \cdot 3} \\
 & \quad \quad \quad \mathcal{E}c. \qquad \qquad \qquad \mathcal{E}c.
 \end{aligned}$$

The difference between which and half ( $w^n$ ), the sum of all the chances, (which difference I shall denote by  $D$ ), will consequently be the number of the chances whereby the errors in excess (or in defect) can fall within the given limit  $m$ : so that  $\frac{D}{\frac{1}{2}w^n}$  will be the true measure of the required probability, that the error, by taking the Mean of  $t$  observations, exceeds not the quantity  $\frac{m}{t}$ , proposed.

To illustrate this by an example, from whence the utility of the method in practice, may clearly appear, it will be necessary, in the first place, to assign some number for  $v$ , expressing the limits of the

the errors to which any observation is subject. These limits, indeed, depend on the goodness of the instrument, and the skill of the observer; but I shall suppose here, that every observation may be relied on to 5 seconds; and that the chances for the several errors,  $-5''$ ,  $-4''$ ,  $-3''$ ,  $-2''$ ,  $-1''$ ,  $0''$ ,  $+1''$ ,  $+2''$ ,  $+3''$ ,  $+4''$ ,  $+5''$ , included within the limits thus assigned, are respectively proportional to the terms of the series 1, 2, 3, 4, 5, 6, 5, 4, 3, 2, 1: which series seems much better adapted than if all the terms were to be equal, since it is highly reasonable to suppose, that the chances for the different errors decrease, as the errors themselves increase.

These particulars being premised, let it be now required to find, what the probability, or chance, for an error of 1, 2, 3, 4, or 5 seconds will be, when (instead of relying on one) the Mean of six observations is taken. Here, then,  $v$  being  $=5$ , and  $t=6$ , we have  $n(=2t)=12$ ,  $w(=v+1)=6$ , and  $p(=tv+n+m)=42+m$ : but the value of  $m$ , if we first seek the chances whereby the error exceeds not 1 second, will be had from the equation  $\frac{m}{t} = \pm 1$ ; where either sign may be used, but the negative one is the most commodious: from whence we have  $m(=-t)=-6$ ; and therefore  $p=36$ ,  $p'=30$ ,  $p''=24$ ,  $p'''=18$ , &c. which values being substituted in the general expression above determined, it will become  $\frac{36}{1} \cdot \frac{35}{2} \cdot \frac{34}{3} (12) - \frac{30}{1} \cdot \frac{29}{2} \cdot \frac{28}{3} (12) \times 12 + \frac{24}{1} \cdot \frac{23}{2} \cdot \frac{22}{3} (12) \times 66 - \frac{18}{1} \cdot \frac{17}{2} \cdot \frac{16}{3} (12) \times 220 = 299576368$ : and this subtracted from  $1088391168 (= \frac{1}{2} \times 6^{12})$ , leaves  $788814800$ , for the value of  $D$  corresponding.

Therefore the required probability, that the error, by taking the Mean of six observations, exceeds not a single second, will be truly measured by the fraction

$\frac{788814800}{1088391168}$ ; and consequently the odds will be as

788814800 to 299576368, or as  $2\frac{2}{3}$  to 1, nearly.

But the proportion, or odds, when one single observation is relied on, is only as 16 to 20, or as  $\frac{8}{5}$  to 1.— To find, now, the probability, that the re-

sult comes within 2 seconds of the truth, let  $\frac{m}{t}$

be made = - 2; so shall  $m (= - 2t) = - 12$ ; and

therefore  $p = 30$ ,  $p' = 24$ ,  $p'' = 18$ , &c. And our

general expression will here come out = 36079407;

and consequently  $D = 1052311761$ : whence

$\frac{1052311761}{1088391168}$  will be the true measure of the probabi-

lity here sought; and the odds, or proportion of the chances, will therefore be as 1052311761 to

36079407, or as 29 to 1, nearly. But the propor-

tion, or odds, when one single observation is relied

on, is only as 2 to 1: so that the chance, for an

error exceeding 2 seconds, is not  $\frac{1}{5}$  part so great

from the Mean of six, as from one single observa-

tion. And it will be found, in the same manner,

that the chance for an error exceeding 3 seconds, will

not be  $\frac{1}{10}$  part so great from the Mean of six, as

from one single observation. Upon the whole of

of which it appears, that the taking of the Mean of

a number of observations, greatly diminishes the

chances for all the smaller errors, and cuts off al-

most all possibility of any great ones: which last

consideration, alone, seems sufficient to recommend

use of the method, not only to astronomers, but to all others concerned in making of experiments of any kind (to which the above reasoning is equally applicable). And the more observations or experiments there are made, the less will the conclusion be liable to err, provided they admit of being repeated under the same circumstances.

Other examples, and particulars might be added, in confirmation of what is here determined; but as I would not appear tedious to your Lordship, I here conclude, who am,

Woolwich,  
March 4, 1755.

My Lord,

Your Lordship's

most obedient humble servant,

T. Simpson.

XX. *An Account of the Success of Agaric,  
and the Fungus vinosus, in Amputations :  
By Mr. James Ford, Surgeon, of Bristol.*

Bristol, March 31, 1755.

Read April 10, 1755. **I**N 1753 I had some pieces of the agaric of the oak brought me from France, which I have frequently used with success in hæmorrhages, particularly once after the operation for the stone, where a large artery lay so deep, that it could not conveniently be taken up with a needle. After the publication of Mr. Warner's observations, Mr. Thornhill applied it successfully to  
an

an amputation of an arm, and a leg, in adult subjects. I had very early suspicions, that this effect of the agaric was more owing to its texture, than any specific stypticity, and therefore determined to try a fungus, very similar to it in substance, that grows in great abundance on the casks, walls, &c. in wine-vaults, and which the coopers here apply to their wounds, whenever they cut themselves. I have inclosed two pieces of this *Fungus vinosus*, one from the cask, which is of a firmer texture than the other, which was taken from a partition-door in the cellar. I made choice of the former in the following experiment.†

A. B. Aged 36, strained his ankle two years ago, from whence an abscess in the joint, and caries, ensued: the pain and discharge brought on many hectic symptoms, particularly a very troublesome cough. On the 5th instant I amputated the leg in the infirmary; and, after the operation, applying my finger to the great artery, unscrewed the tourniquet, to discover the small ones, which the assistants covered in like manner. I then screwed it to such an exact degree of tightness as to stop the bleeding from the large artery, which I spunged very dry, and applied a thick bit of the fungus, with some lint over it, and so to each of the others, and dressed it up, as usual, with a slight bandage. In five minutes I began to unscrew the tourniquet, and by slow degrees (so that the blood might not rush too suddenly on the extremity of the artery), in half an hour loosened it intirely. In unscrewing the tourniquet, I kept an account how many turns I made; that, if the artery had bled, I might be able to determine what exact

exact degree of tightness was necessary to compress it again. Four hours after the operation, he had occasion to use the bed-pan, and his efforts to raise himself made it bleed so as just to stain the roller, but it stopped without doing any thing to it. On the seventh he had a violent fit of coughing, followed by a vomiting, which renewed the bleeding, and it bled about three or four ounces, but before any one could come to his assistance, it stopp'd: however, the tourniquet was screw'd so as to make a slight pressure on the femoral artery for a few minutes, when it was loosened again. On the 10th the stump was opened; it had a very fair appearance, and has gone on very well. The fungus did not drop off from the great artery till the 17th.

March 15, Mr. Thornhill performed an amputation, in the infirmary, above the knee, on a man of twenty-one years of age, and applied this fungus. On the 20th it was opened, without the least stain of blood; but on the 22d it bled, from a fit of coughing: tightening the tourniquet for a short time stopped it, and it has not bled since. The fungus adhered to the wound till the 30th.

James Ford.

XXI. *Queries sent to a Friend in Constantinople; by Dr. Maty, F. R. S.; and answered by his Excellency James Porter, Esq; his Majesty's Ambassador at Constantinople, and F. R. S.*

*The Queries proposed were the Seven following :*

Read April 10, 1. <sup>1755.</sup> **W**Hether we may know with any certainty, how many people are generally carried off by the plague at Constantinople?

2. Whether the number of inhabitants in that capital may be ascertained?

3. Whether what has been advanced by some travellers, and from them assumed by writers on politics, be true, that there are more women than men born in the east?

4. Whether plurality of wives is in fact, as it was confidently affirmed to be, *in the order of nature*, favourable to the increase of mankind?

5. What is the actual state of inoculation in the east?

6. What is become of the printing-house at Constantinople? and are there any original maps of the Turkish dominions, drawn from actual surveys?

7. What sort of learning is cultivated among the Greeks, and among the Turks?

*To these Queries his Excellency James Porter, Esq; his Majesty's Ambassador at Constantinople, and Fellow of this Society, was pleased to make the following Answer:*

1. The only plague, which I observed at Constantinople, in the course of seven years, was that of the year 1751: there are almost annually dispersed accidents, some perhaps real, some suggested by trick and design, to serve sinister purposes.

I attempted that year to throw some observations on paper; but all that I could make out of them was so unsatisfactory, trite, and imperfect, that I thought them, on a review, scarce worth notice.

I am convinced, that whatever is told us of that distemper is scarce to be depended on; rather conjecture than observation, rather the play of imagination than fact. However, I had made it a principal study to attain to some data, whereby I could draw a probable conclusion concerning the number of the dead, that year, which might also have led me to have ascertained that of the inhabitants at Constantinople.

2. The Turks have no register, no bills of mortality: they are prohibited, by their law, to enumerate the people. I applied to the Reis Effendi, and other ministers of the Porte, to know what probable calculation they could make concerning the number of dead; but they all concurred in one general answer, that they had no other but what was founded on the decrease of the consumption of the quantity of corn, or bread; and in general talked of about 150000. I therefore betook myself, with all my



care and industry, to try what probable conclusions I could draw from that imperfect datum. Corn is delivered out by an officer of consideration, and an exact register kept.

Before the commencement of the plague, that was in March and April 1751, the consumption of corn was 19000 measures, called Khilo's. On its continuance and decrease it diminished to 17000, and on its total cessation, it was found not to amount to above 14000. A khilo weighs twenty-two okes. It is grounded to eighteen okes of flower. The bakers have generally the secret to make out of this last quantity twenty-seven okes of bread. They add to an oke of flower one of water, besides some salt; and as their bread is almost dough, few of the watry particles are exhaled; and it is thought of the best, if it is not doubled in quantity, when taken out of the oven.

The common people, and even most of the middling and easy, live principally on bread; the former with onion, garlick, fruits, or pulse, according to the seasons; the latter with very small portions of flesh, or fish. The more laborious professions, as labouring men, stone-cutters, carpenters, &c. eat from two to two and a half okes a day; the other, according to the common run of families, composed of men, women, and children, half an oke each; so that the lowest calculation, on a medium, may be about an oke and a quarter daily, eaten by each person at Constantinople.

But should it be thought too much, an oke, which is two pounds three-quarters English, we may suppose

pose nearer truth : the following conclusions then will result :

That therefore on the highest number of 19000  $\times$  27, we have 513000, the quantity of okes of bread consumed, and consequently the number of souls at Constantinople. That on the decrease of the plague to 17000, 54000 persons were either dead or missing. That when the quantity was reduced to 14000 on the cessation, those either fled or dead amounted to 135000.

It is said by some, that Constantinople contains near three millions of inhabitants; but on whatever supposition we take the consumption of the quantity of bread, that quantity will be found erroneous.

On a gross calculation made by some of the principal men, and particularly the Chiorbachee's, or colonels of Janizaries, who had their stations at the most noted and only places where the funerals pass, they reckoned for six weeks, whilst the plague was at its height, and in its crisis, from 900 to 1000 *per diem*; and that the whole amount of the dead in that time might be about 40000: and from the time it was in its increase and decline, they added 15 to 20000 more. If therefore we admit 60000 in the whole, it will be as that sum to 513000, or as 1 to  $8\frac{1}{5}$ .

There is a remarkable coincidence between this proportion, and the number of dead which was carried out of the Adrianople-gate, during twelve days, the same season of the year 1752; and of the like number of days in 1751.

1752 Health.		1751 Plague.	
June	14———11	June	12———24
	15———3		13———29
	16———5		14———32
	17———5		15———35
	18———4		16———24
	19———8		20———50
	20———3		25———34
	22———3		22———37
	23———5		23———52
	24———4	July	14———56
	25———5		15———57
	26———3		16———59
	<hr/>		<hr/>
	59		489

So that the number of dead; at least through that gate, in time of common health, was to those in that of sickness, as 59 to 489, or as 1 to  $8\frac{1}{3}$ , nearly.

The Adrianople-gate is reckoned the greatest passage for the dead, on account of its vicinity to the most extensive burial-places.

A great deduction must be made for the vast decrease of the consumption of wheat towards the cessation of the plague, from the considerable numbers, who fled into Asia, the islands of the Archipelago, and Romelia.

A cogent argument with me to demonstrate, that Constantinople is not peopled in proportion to its extent, is the immense care which the late Sultan Mahmud took, not to admit new inhabitants, or strangers ;

strangers; none could remain a night in the city without commands from the Porte; and those were with great difficulty obtained.

It is extreme difficult, if not impossible, to come at any other *computus* of the number of inhabitants, much more so of houses, at Constantinople. The city is divided somewhat in the manner it was under the Græcian empire, that is, into different quarters, called Mahales, and each under the special direction of an Imaum. As far as it extends to their immediate advantage, they are informed of the number of families in their district; but whoever would dare to collect from them, might not only risk the censure of the government, but his head. Besides, if the inquiry is general concerning houses, it is impossible to fix a determined idea; they confound palace, seraglio, shop, room, and call them indiscriminately houses. The Jews say, that they have 10000 houses at Constantinople: but in what we call a house, there are perhaps ten families, and the distinct number of the latter they dare not mention. I endeavoured with persuasion, and all my weight, to induce the Greek and Armenian patriarchs, to obtain for me a register of the births and burials of their respective communities; but at length they acknowledged it impossible. Their parishes are farmed to curates, by the diocesan bishops; the income arises from births and burials; so that to conceal the former, they must likewise the latter; and they never exhibit a faithful register.

3. That there are more women than men born in the east, seems a figment of travellers, rather than founded in truth; it is scarce to be known, where  
polygamy

polygamy is lawful. The apparent conclusion may seem natural, because many of the Harems of the opulent, especially in the great cities, are numerous: but these are not composed of the natives of those cities, but are brought from countries where the christian rites are observed, in time of peace, from Georgia; and of war, from Hungary and Russia, &c. so that if more women are found in such families than men, they must be looked upon as an extraordinary production annually, or daily imported.

I think I can aver, on good foundation, that we have not yet extant, an exact genuine account of the customs, manners, and practices, of these people, nor really of these countries. Those which I have read are extreme faulty, not to say worse, in many particulars, which have fallen under my own knowledge. What am I then to conclude, as to those that have not? And how can a Tournefort, and many others I could name, in running over vast tracks of countries in two years, or less, sometimes by night, sometimes by day, with hasty caravans, give us a true history? Even Ricaut's, he, who dwelt some years in these countries, is founded on very imperfect memoirs: what he says of the interior of the seraglio is impossible to be known; and I find by some original letters of his, from Hamburg, to a nephew he had, as secretary to this embassy, that he begged for some notes of one Mr. Coke, formerly in that office, in order to continue his history. Now, it is evident, that all such notes must have been only the hearsay of the Christians of Pera, who neither have, nor ever had, knowledge or observation sufficient to be depended on; nor dare they venture to  
enter

enter into intimate particulars with such Mahometans as could truly inform them.

Credulity and vulgar errors abound, as a consequence of their faith ; for they are all Greeks or Romanists. Those are taken traditionally. To instance in one collected from them, and universally received by travellers ; they tell us, that the Turks make public prayers and processions in time of plague, when 1000 corps a day are carried out of the Adrianople-gate. This tradition was current in the year 1751. I knew it must be false from the very Koran. However, I had the question put to many, particularly to the Reis Effendi, great chancellor of the empire, who let me know they never numbered them, nor inquired minutely how many died ; that in time of great calamity or sickness, they only ordered a passage of the Koran to be read in their Mosches. Thus, by a single inquiry, I detected a fable which has passed current, since Mahomet the Second's time. This is one example of many.

4. I can affirm, with truth, what may seem a paradox, that in general, Mahometans, notwithstanding their law, procreate less than Christians. The rich, who are the only persons that can maintain concubines, have seldom four or five children. Few, I have heard of, or known, exceed two or three ; many of the former, and most of the middling and poorer sort, have generally but one wife. The latter indeed exchange them with facility ; but yet we do not perceive they have a numerous progeny. I take this to arise from a cause different from that which is commonly assigned, not from their being enervated by variety, but rather from their law. The frequent ablutions

ablutions, required by the doctrine of purity and impurity, perhaps may check the libidinous passion; or when it is at its height, they find themselves prohibited enjoyment. To enforce this observation might lead me into some singular reflections.

5. Inoculation is practised at present among the Greeks, and, notwithstanding religious scruples, among the Romanists: with the few, whom I have known, it generally succeeded; but the numbers will not admit of comparison. There are not perhaps twenty in a year inoculated. The Timoni family pretend, that a daughter had been inoculated at six months old, but afterwards acquired the small-pox in the natural way, and died at twenty-three years. The evidence is doubtful. Timoni's account is incorrect; his facts are not to be depended on. Pylarini's is more exact. It was neither Circassians, Georgians, nor Asiatics, who introduced the practice. The first woman was of the Morea; her successor was a Bosniac; they brought it from Thessaly, or the Peloponnesus, now Morea. They properly scarified the patient, commonly on many parts, sometimes on the forehead, under the hair, sometimes on the cheeks, and on the radius of the arm. A father told me, that the old woman not being able, through age, to make the incision on his daughter, with the razor, he performed that operation. The needle has also been used. The Turks never inoculate: they trust to their *fatum*. Whence the method had its origin seems here unknown. A Capuchin friar, whom I often see, was on a mission in Georgia for above sixteen years; he has returned about two years; he is a grave sober man, who gives an historical account of the virtues and vices, good  
and

and evil, of that country, with plainness and candour. The usual introduction and security of these missionaries is the pretence to the practice of physic, that in destroying bodies they may save souls : so that this honest man, who is extremely ignorant, was in high reputation both as physician and confessor. It was therefore impossible, as he himself observes, that either the public or private practice of inoculation could be concealed from him ; but he has most solemnly declared to me repeatedly, that he never heard one word about it at Akalsike, Imirrette, or Tifflis ; he is persuaded, that it has never been known among them. He has often and frequently attended the small-pox, which is almost certain death there ; and generally, if not always, of the confluent kind.

6. Printing was introduced by an Hungarian renegado, who called himself Ibrahim Effendi : it had no long continuance. The copies are not many, and are now very dear and scarce ; few even to be bought.

The maps did not exceed three or four ; one of Persia, one of the Bosphorus, and one of the Euxinus, or Black-sea ; they are not to be found but in private hands. All our maps of these countries are extremely imperfect and incorrect.

The jealousy and superstition of the people, tho' the government should permit Christians to raise any printing-house, would be an irresistible impediment ; and they are too ignorant themselves to be ever capable of doing it. The adoptive son of this Ibrahim Effendi, who bears the same name, is secretary under the interpreter of the porte ; he has all the



materials for printing, but never could find, since his father's death, and during Sultan Mahmud's reign, money to carry it on. The question is now, whether Sultan Osman is not too strict a mussulman to continue the permission.

7. The progress of arts and sciences, and literature, seems travelling on, *gradatim*, to the westward, from Ægypt to Greece, from Greece to Rome, thence to the west of Europe, and I suppose at last to America. We find few traces in the east: the Greeks, who should be the depositaries of them, are the same Greeks they ever were, *Homines contentiosis cupidiores quam veritatis*. They have retained all the vices, imperfections, ill habitudes, of their ancestors; but have lost all their public spirit, and public virtues. The clergy, who should support the whole machine of learning, are themselves the source of ignorance; all their talents and acquisitions consist in bribing amongst the Turks, and soliciting to destroy one patriarch in order to make another; to raise from a curacy to a bishoprick, and to exchange from an indifferent one to a better. They endeavour to cultivate literal Greek, and some study it, but advance no further. There are neither grammarians, critics, historians, nor philosophers, amongst them; nor have they proper preceptors or masters to instruct. They have formed a sort of an academy at Mount Athos, for their youth, which will scarce survive the person who has undertaken it: he has himself but the mere elements of science. However, his desire of knowing may improve him; and he may perhaps lay the foundations in some youth with success.

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The Turks have many books amongst them, tho' exceedingly dear; folios I have seen cost 100 to 2 and 300 dollars each; *i.e.* from 15 *l.* to 45 *l.* The few printed folios, some of which I pick'd up some years ago, cost 5 *l.* to 6 *l.* sterling. Their scribes spend many years about a few copies. Their learning consists principally in abstruse metaphysics: some few touch the surface of science. I have looked out with great industry for old Arabian manuscripts in the mathematical way: what they brought me were translations of some propositions of Euclid, Theodosius, Archimedes, and Apollonius. They have some parts of Aristotle; but their favourite philosophy is the atomical or Epicurean, which with them is called the Democritical, from Democritus. Many of their speculative men have adopted that system, and conform to it in their secret practice. The institutes and practice of physic are taken from Galen. Eben Zyna, or Avicena, is a principal guide: Mathiolus is known. But with all this, as the sole drift and end of their study is gain, there does not seem the least emulation towards true knowledge: so that the state of letters may be said to remain deplorable, without the least glimmering, or remote prospect of a recovery.

Constantinople,  
Feb. 1, 1755.

J. P.

Since the reception of this memoir, Dr. Maty has received another letter from the same gentleman, in which he finds some new facts tending to clear up the accounts relating to the practice of inoculation among the Georgians. These he hopes will not be unacceptable, as they come from a person equally able, by his universal knowledge and distinguished station, to procure the best informations, and willing, for the good of mankind, to communicate them in the most obliging and candid manner.

Constantinople, May 17, 1755.

**I** AM now to correct the report of the Capuchin concerning inoculation in Georgia. One of their physicians, a most ignorant fellow, who lives by his profession here, avers that, among those who follow the true Georgian rites, not Romanists, the practice is common. It has its rise from mere superstition. He tells us, " That the tradition and religious belief of that people is, that an Angel presides over that distemper, that therefore, to shew their confidence in him, and to invite him to be propitious, they take a pox from the sick person, and, by a scarification, they insert it in one in health, generally between the fore-finger and thumb. It never misses its effect, and the patient always recovers. To attract the Angel's good-will more effectually, they hang the patient's bed with red cloth or stuff, as a colour most agreeable to him. He has been assistant to this practice, and declares it to be common." Perhaps the only good effect ever known produced by that monster superstition ! The Capuchin acknowledges, that it might be amongst  
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the Georgians the Doctor mentions, and not have fell under his knowlege. To vindicate his ignorance, he distinguishes the parts of Georgia, or its divisions. He has never been at Cackett, and staid but three weeks at once in Tifflis.

J. P.

XXII. *Extracts of Two Letters to Thomas Hollis, Esq; concerning the late Discoveries at Herculaneum.*

Read April 10,  
1755.

ALL the antiquities, which were in the royal palace at Naples, that is, those which did belong to the Parma collection, have been carried to the King's new palace of Capo di monte; and it is reported, that Padre Pancrazi will have the care of them.

Paderni has the custody of all the bronzes, and things in gold and silver, which have been found in the different places where they have dug, by order of the King; and they are handsomely arranged in several rooms at Portici. The great gallery there is almost finished. In it are to be deposited the statues in bronze and marble, the pictures, the inscriptions, and the columns of verd'antique and oriental alabaster found throughout the kingdom.

Near the royal palace at Portici, has been discovered a large garden, with a palace belonging to it. In one room of this palace was found a mosaic pavement (which I have seen) made up of different coloured stones. It represents a city surrounded with walls,

walls, having four towers, one at each corner; and has since been taken up, to be placed with other beautiful antique pavements in the said gallery.

For some time past they have been digging at Santa Maria di Capua, by the King's order. There they have met with several very fine statues of Greek workmanship; and among them a Venus, which is intire, and matchless; and all of them have been carried to the King's new palace at Caserta.

*Extract of a Letter from Camillo Paderni,  
dated at Naples, January 1755.*

Read April 10, 1755. **O**CTOBER 22, 1754, was found a bust in bronze, larger than the life, and of excellent Greek workmanship; which from some circumstances may be thought to be a Syrian king. It has eyes of white marble, like many other busts, which have been met with.

November 27, we discovered the figure of an old fawn, or rather a Silenus, represented as sitting upon a bank; with a tyger lying on his left side, upon which his hand rested. Both these figures served to adorn a fountain, and from the mouth of the tyger had flowed water. This Silenus was of bronze, and of good workmanship. The head was crowned with ivy, the body all over hairy, and the thighs covered with a drapery.

From the same spot were taken out, November 29, three little boys of bronze, of a good manner. Two of these are young fawns, having the horns and ears of a goat. They have likewise silver eyes, and each  
of

of them the goat-skin on his shoulder, wherein they antiently put wine, and through which here the water issued. The third boy is also of bronze, has silver eyes, is of the same size with the two former, and in a standing posture like them, but is not a fawn. On one side of this last stood a finial column, upon the top of which was a comic mask, that served as a capital to it, and discharged water from its mouth. All the figures before described are two palms in height without their bases.

December 16, in the same place were discovered another boy, with another mask, and three other fawns; in all respects like those, which were found the 27th and 29th of November, except that there was no tyger. Besides these we met with two little boys in bronze, somewhat less than the former. These likewise were in a standing posture, had silver eyes, and held each of them a vase, with handles, upon his shoulder; from hence the water flowed. We also dug out an old fawn, crowned with ivy, having a long beard, a hairy body, and sandals on his feet. He sat astride upon a large goat skin, holding it at the feet with both his hands, from which had issued a larger quantity of water than from the others; though the fawn himself is of the same size with the former.

All the above-mentioned figures were taken out of a place not exceeding eight palms square, and were covered with the ruins of the building: for they were not in a garden, but in a room paved with mosaic work, the remaining part of which we are now going on to examine. You may rely intirely upon what I write, as nothing can be moved from  
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the place where it is discovered, but in my presence. We have likewise found a large quantity of household furniture, made of earthen and iron ware, and some glafs. At present this is all that I am at liberty to mention. Shortly will be published a general catalogue of all the things, which have hitherto been found; and this year will come out also the first volume of the paintings. Both these I will take care to convey to you.

XXIII. *Copy of a Letter from a learned Gentleman of Naples, dated February 25, 1755, concerning the Books and antient Writings dug out of the Ruins of an Edifice near the Site of the old City of Herculaneum; to Monsignor Cerati, of Pisa, F. R. S. sent to Mr. Baker, F. R. S. and by him communicated; with a Translation by John Locke, Esq; F. R. S.*

Read April 17, 1755. **I**N obedience to your commands, I send you the best account I can of the writings. You must know then, that within two years last past, in a chamber of a house, (or more properly speaking, of an antient villa, for by many marks it is certainly known, that the place, where they are now digging, was never covered with buildings, but was in the middle of a garden), there has been found a great quantity of rolls, about half a palm

palm long, and round; which appeared like roots of wood, all black, and seeming to be only of one piece. One of them falling on the ground, it broke in the middle, and many letters were observed, by which it was first known, that the rolls were of papyrus. The number of these rolls, as I am told, were about 150, of different sizes. They were in wooden cases, which are so much burnt, as are all the things made of wood, that they cannot be recovered. The rolls however are hard, though each appears like one piece. Our king has caused infinite pains to be taken to unroll them, and read them; but all attempts were in vain; only by flitting some of them, some words were observed. At length Signor Affemani, being come a second time to Naples, proposed to the king to send for one father Antonio, a writer at the Vatican, as the only man in the world, who could undertake this difficult affair. It is incredible to imagine what this man contrived and executed. He made a machine, with which, (by the means of certain threads, which being gummed, stuck to the back part of the papyrus, where there was no writing), he begins, by degrees, to pull, while with a sort of engraver's instrument he loosens one leaf from the other (which is the most difficult part of all), and then makes a sort of lining to the back of the papyrus, with exceeding thin leaves of onion (if I mistake not), and with some spirituous liquor, with which he wets the papyrus, by little and little he unfolds it. All this labour cannot be well comprehended without seeing. With patience superior to what a man can imagine, this good father has unrolled a pretty large piece of papyrus,



pyrus, the worst preserved, by way of trial. It is found to be the work of a Greek writer, and is a small philosophic tract (in Plutarch's manner) on music; blaming it as pernicious to society, and productive of softness and effeminacy. It does not discourse of the art of music. The beginning is wanting, but it is to be hoped, that the author's name may be found at the end: it seems however to be the work of a stoic philosopher; because Zeno is much commended. The papyrus is written across in so many columns, every one of about twenty lines, and every line is the third of a palm long. Between column and column is a void space of more than an inch. There are now unrolled about \* thirty columns; which is about a half of the whole; this roll being one of the smallest: the letters are distinguishable enough. Father Antonio, after he has loosened a piece, takes it off where there are no letters; and places it between two crystals for the better observation; and then, having an admirable talent in imitating characters, he copies it with all the lacunæ, which are very numerous in this scorched papyrus; and gives this copy to the Canon Mazzocchi, who tries to supply the loss, and explain it. The letters are capital ones, and almost without any abbreviation. The worst is, the work takes up so much time, that a small quantity of writing requires five or six days to unroll, so that a whole year is already consumed about half this roll. The lacunæ, for the most part, are of one or two words, that may be supplied by

\* Original—*Trenta*.

the context. As soon as this roll is finished, they will begin a Latin one. There are some so voluminous, and the papyrus so fine, that unrolled they would take up an hundred palms space. They tell me, that some of the Latin ones are in a running hand; which confirms the opinion of the Marquis Maffei, "That the character, by us absurdly called Gothic and Lombard, is the antient running-hand, corrupted by time." However, I have not seen any of these last. The curiosity of these papyri is, that there is no little staff of wood, on which they were rolled.

Thus I have told you all, that I know, concerning these papyri.

We may comfort ourselves, that the affair is in good hands; being under the care and conduct of so learned an antiquarian, as the Canonico Mazzocchi, and of this able and adroit Father Antonio.

XXIV. *An Account of the several Earth-  
quakes of late felt at Constantinople; by  
his Excellency James Porter, Esq; his Ma-  
jesty's Ambassador at that Place; in a  
Letter to the Reverend Mr. Wetstein,  
F. R. S. dated February 15, 1755.*

Read April 17, 1755. **H**istorians of the lower empire give us various accounts of earthquakes, which have happened at Constantinople. Gillius informs us of many of later date; and an ancient co-

lumn standing in the city, called the burnt pillar, declines from the perpendicular, as a testimony of some considerable shock. The only memorable one I have heard mentioned was in the year 1719, which seemed so rather from the continuation of several shakes for thirty days, than any considerable damage.

Some inconsiderable, and scarce meriting notice, I have felt in different seasons; one very sensible on the  $\frac{1}{2}\frac{5}{6}$ th of May 1752, at five in the morning.

We had an account of a violent earthquake at Adrianople that year, on the  $\frac{1}{2}\frac{8}{9}$ th of July, at about eight p. m.; that it was attended with uncommon fissures and openings of the earth, and eruptions of waters, carrying a sulphureous stench. Considerable damage was done to many mosques and houses: the ruins remain to this day. Lesser shakes were felt the whole month of August. We did not hear, whether it extended to the westward, nor of any particular phenomena preceding it.

On the same day and hour we had it very strong at Constantinople; it lasted some seconds. I mark'd the wind at south that morning, and the spirits in the Thermometer at 40: the mercury in a small mercurial Thermometer of Bird's stood at 79. The afternoon the wind came about to the E. S. E. a strong gale, which continued during the earthquake. Its first motion appeared to us perpendicular, and a heaving of the house, and us, as it were, off of our chairs. It was succeeded by three or four regular momentaneous horizontal vibrations, so that the walls of the house receded and returned, like the reciprocal motions of a ship; and it was with difficulty we stood firm. These vibrations we judged had their  
direction

direction from N. W. to S. E. and their origin from Adrianople. The Thermometer at that time stood at 36; the mercury at 81. A dead calm succeeded, and at 11 at night I marked the spirits 37, and mercury 80. The letters from Smyrna told us, that a small shake was felt there at the same hour: so that if we could suppose all the clocks and watches exact, and that the first explosion of the igneous matter was at or about Adrianople, we must conclude the communication of these vast distant motions to have been instantaneous; and though we should vary some minutes, the velocity is incredible, and may be thought nearly to come up to that of sounds.

We had on the 9th of November, that year, a small shake, at 5. 30'. a. m. Spirits 68. a. m. 66. p. m. Merc. Ther. 56. a. m. 54. p. m. Wind N. E. fair weather.

The most violent one I ever felt, was on the 2d of September 1754, at 10 at night. The preceding month did not vary 4 or 5 degrees of heat from other years: it came, as that of the 29th of July, without any preparatory signs, and with this difference only, that it was in a dead calm. The wind that day had been from E. N. E. to E. the sky at night with short clear undulating clouds, some bluer than others, and a bright moon. The first shock seemed intirely perpendicular, and the house, with a violent crack, heaved as it were off of its foundation; so that the floor of a large chamber seemed to rise from the exterior wall of the house to the interior of that room, and made a perfect inclin'd plane. The windows appeared to me higher, and the chair, in which I sat near that interior wall, to sink lower; and when I rose up I perceived

perceived the inclination very sensibly. It settled soon after, and was succeeded by several horizontal vibrations: all the pendulums stopp'd; mine precisely at ten, some at a few minutes after or before, according to their greater or less regularity. My servants told me, that the horses rose from their litter with violent emotions, and continued some time uneasy: all the fowls were on the wing, and cackled; dogs and cats ran about seiz'd with fear. The wells, of which I have three, became the one dry, the others almost so. The damage was considerable, but mostly amongst the old stone or brick buildings, as mosques, the seven towers, public khans, and old walls. All the houses are built with wood and mud brick dried only by the sun, which the people of the country pretend to be, by the less weight on the foundations, a greater security against earthquakes: in fact, none but very old ones, and but few suffered; those well repaired came off unhurt, or with some cracks and fissures only. It was immediately said, that the persons killed amounted to some hundreds; but the truth is, there were not above sixty. The more elevated the apartments, the more sensible the shock; those, who were in the streets, or in the fields, in motion, did not feel it. The duration by many was extended to minutes; but, as far as I could judge, it did not exceed some seconds, at most 30". The Thermometer at 48. a. m. 47. p. m. the Mercury at 74. a. m. 72. p. m. and the Mercury in the Barometer at 2. Tolerable good weather.

We perceived its line of direction from the east, from whence we had afterwards an infallible confirmation; for it has been traced from Sivas, a large city

city in Asia Minor, towards the confines of Diarbeckir, or Mesopotamia, where it has done some damage: thence it came on west, and left great ruins in some small towns and villages; passed to Nicomedia, and so to Constantinople. We had no account of its extending to Adrianople; it was merely felt at Smyrna; other shakes were felt that night at twelve of the clock.

And on the 3d, at 10 and 12 a. m. wind at E. and S. E. Ther. 46. a. m. 44. p. m. Mercurial 75. a. m. and p. m.

On the 4th, two more, one at 2 and at 11. 15. p. m. wind N. and N. E. fresh. Ther. 42. a. m. 44. p. m. Merc. 78. a. m. 75. p. m. Bar. 1. mark'd dark weather, wind N.

The 5th, one at the break of day, and another at 9. 40'. a. m. wind E. N. E. to N. fresh clouds. Ther. 44. a. m. 45. p. m. Mer. 75.

On the 6th, two shakes at four in the morning. Little wind, almost calm. Ther. 44. Mer. 77. a. m. 76. p. m. At between 8 and 9 of the clock this night we had a singular phenomenon; a topical oblong cloud formed itself due west in the very point where the sun had set, of a black dense consistency, emitting flames, rays, and fiery corruscations, direct and transversal, sometimes stellate, like stars, sometimes like laminæ or blades, intermixed with seeming blue or sulphureous steams: precisely at 9 there issued from it a hollow rumbling sound: the cloud suddenly broke forth, as it were, from a fix'd point, and by an equal flow motion traversed the whole northern hemisphere, which was before, and remained after, extremely bright and clear; the noise  
continuing,

continuing, as it approached us, like one uninterrupted confused peal of thunder, accompanied first with a shower of hail, and soon after converted into a heavy rain. It lodged itself due east, where it continued emitting flame, and diminishing; until at about 10. 30'. it seemed to have consumed itself.

If this collected mass of igneous steams and vapour had freed us from earthquakes, it might be presumed to have had some immediate connection with them. However, it may be worth observation, whether such an evaporation might not diminish the quantity of that combustible matter in the interior of the earth, and consequently the force of the explosion and tremulous motion.

On the 7th, wind S. W. and W. some rain. Ther. 47. a. m. 51. p. m. Merc. 73. a. m. 70. p. m. we had none, at least that we sensibly perceived. Bar. 1. and 0. changeable.

The 8th, wind N. W. and N. rain at night, and some in the morning, at 4. 30'. and at 10 of the clock, a. m. we had two shakes. Ther. 52. a. m. 55. p. m. Merc. 69. a. m. 67. p. m. Bar. at 0. changeable.

The 9th, fair and calm, wind N. E. and E. N. E. Ther. 54. a. m. 53. p. m. Mer. 68. we felt none Bar. 0.

10th, wind at N. and N. N. E. Ther. 52. a. m. 50. p. m. Mer. 70. B. at 1. marks dark heavy weather, and N. wind. we had two earthquakes, one at midnight, and the other at four in the morning.

The 11th, wind E. and N. E. Ther. 50. a. m. 52. p. m. Merc. 71. a. m. 69. p. m. Bar. 2. marks passable weather, an earthquake at 12. 30'. at night.

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The 13th, wind E. fair. Ther. 49. a. m. 51. p. m. Merc. 72. a. m. 70. p. m. Bar. 2. an earthquake at three in the morning.

Though I have noted only those, which for the most part I felt, there may be two or three founded on such reports, as I could fully depend on. Most people pretend, that all this month, at one hour or other, there were continued shakes, and with little intermission. The wind was for the most part of that time at N. E. the spirits falling, and cold increasing.

On the 6th of October, after we had almost forgotten our earthquakes, we were again reminded of them at 8. 45'. p. m. a dead calm. There were several strong horizontal vibrating shakes, the motion undulating equally, without any previous violent crack or noise, or any other preparatory sign, the wind blowing the preceding part of that day at N. E. and E. N. E. the Ther. at 72. a. m. 69 p. m. Mer. 53. a. m. and 55. p. m. Bar. at 0 changeable.

On the 7th, wind E. N. E. fair. Ther. 68 a. m. 69. p. m. Merc. 57. a. m. 59. p. m. a small shake was felt at 12 of the clock at noon

From that time wind variable, but principally hanging towards the N. and N. E. Ther. varying from 62 to 66, we felt none until the 4th of November. We had then a short one at 10. 19'. at night, wind N. E. fair, with light clouds. Ther. 66. a. m. 63. p. m. Mercurial 57. a. m. 58 p. m. Bar. c. changeable. Another was sensible to many on the 19th following, at 9. 45'. at night; but as I was in motion, I did not perceive it. That day heavy clouds, and cold. Ther. 72. Mercurial 53.



From the 15th of December to the end of the month, the weather has set in colder, and more snow and frost than has been known for several years. The spirits varied from 80. great cold to 90. and the Mercurial from 45 to 38. During the month of January, from 88 to 94, 95, and 98. and the Mercurial from 35 to 30.

From the 15th of January to the 20th, the wind varied to S. W. and S. E. the Ther. at 88. Merc. at 38. rose gradually to 81. and 45. accompanied with a thaw.

On the 20th, the wind came about strong, blowing hard about E. N. E. mizzling rain, mix'd with sleet, without consistency. We had three strong vibrating shakes of an earthquake at 12. 34'. Noon, Spirits at 83. Merc. 42. and Bar. at between 1. and 0. tolerable good weather, and changeable; and tho' the cold greatly increased, another was felt on the 23d at 10. 30'. a. m. the snow falling thick, hard frost, wind N. E. Spirit 91. and Merc. 36.

The beginning of February, snow continued, and cold augmented; we have had the greatest known, at least remembered here. The 4th the Spirits sunk under 100. and the Merc. Ther. at 28. The canal, which separates Pera from the city, was frozen over, from the arsenal up to the fresh-water river.

From these observations it doth not appear, that there are any fixed or probable prognostics of earthquakes; but that they come on us indiscriminately in the midst of high winds and calms, heat and cold, rain, snow, and fair weather; so that no other connexion can be suspected of these with the atmosphere, than merely the collected mass of igneous exhalations, perceived on  
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the 6th of September at night; unless the direction of the winds, which seemed most commonly nearly in the same line with the shakes of the earthquakes, might be thought to have any.

2dly, What some of the ancients have told us of the spring and autumn being the two usual seasons for earthquakes, appears not only from these, but what has been observed by others, to be no general secure rule, since they happen equally in all seasons.

3dly, The velocity of motion, and the distance of the line of communication, appear extremely surprising. From Adrianople to Smyrna, in a right line, is not less than 250 miles, and to Constantinople 150 miles. Possibly the reason it was felt with less force at the former of these places arises from the difference of distance, and that its force decreased in proportion to it; whence we might form a conjecture on some grounds, that the origin of the explosion was at or about Adrianople.

Sivas, in a right line, I compute at about 750 miles. I cannot yet be informed of the hour or precise time when they felt it there: I heard that it was on the same day. These people are not sufficiently observing to remember to an hour.

*N. B.* The Thermometer is graduated from 1. to 100. descending, 50 temperate, 60. to 65. cold, 80. freezing point, 95. to 100. extreme cold, greatest heat in this climate 35. to 30. not durable.

XXV. *Letters of Henry Eeles, Esq; concerning the Cause of the Ascent of Vapour and Exhalation, and those of Winds; and of the general Phænomena of the Weather and Barometer.*

To the Rev. Tho. Birch, D. D. Secret. R. S.

L E T T E R I.

S I R,                      Lismore (in Ireland), Nov. 25, 1754.

Read Jan. 23, 1755. **I** Received your letter of the 25th of November 1752, in answer to my letter to the Royal Society concerning the cause of thunder. As you tell me, that they honoured me with their desire of my thoughts on some subjects mentioned therein, I have ventured to send you a letter, for them, on one of those subjects, inclosed in two packets, by this post. I fear, that the Society will think my letter too hypothetical; our great modern philosopher having, in some measure, condemned the use of hypotheses. But I must boldly observe, that that objection is made in a part of his works, which is intirely hypothetical: I mean his queries at the end of his third book of optics. And I shall venture to shew (if the Society will have patience to hear me), that there is a great deal hypothetical in his second book of optics, where he thinks himself more ascertained. For he there speaks pretty positively of four causes of the refraction, &c. of light; three of which must be wrong; and the fourth (his æther),

æther), in the manner he has proposed it, can never be sufficient for that purpose. Not that I intend impudently to blame that truly great man, but to endeavour to investigate the cause of reflection and refraction of light, by means not made sensible to mankind in his time. I may also observe, that, if his hypothesis had not preceded his calculation, the world had never been obliged to him for his great doctrine of attraction: for at first he only supposed, that the power of gravity may extend to the moon; and that she may be retained in her orbit thereby: but when he made his calculation (having mistaken sixty English miles for the measure of a degree on the surface of our earth), he thought, that some other power must co-operate with that of gravity, to keep the moon in her orbit; and for that time laid his attempt aside; till Picart, in France, measured a degree, and found, that it contained about sixty-nine English miles and an half; and then, or some years after, our great author having resumed his computation, found, that the power of gravity was alone sufficient to keep the moon and planets in their orbits; and thereby proved his hypothesis.

Indeed, in works of invention, I cannot see how it is possible to proceed without something hypothetical; for the supposition must ever precede the proof. I am not for establishing hypotheses as principles; but if an hypothesis is rationally founded, I think it is worth the while to inquire into the truth of it; and thus invention may proceed.

I shall not say any thing in justification of my own, because I have not any vanity to gratify; for if my letter has not any thing worth the Society's or your acceptance,

ance, I can most readily and willingly ask pardon for the trouble, which I have given, and desist from giving any for the future. I am,

S I R,

Your most obedient humble servant,

Hen. Eeles.

*To the Royal Society.*

L E T T E R II.

Gentlemen, Lismore (in Ireland), Nov. 25, 1754.  
 Read Jan. 23, and Feb. 6, and 13, 1755.

**T**HE great honour you have done me, in accepting my letter concerning the cause of thunder, has emboldened me to proceed in part of what I therein proposed, *viz.* to endeavour to shew, that the fire, which is made apparent by electrical experiments, is the principal cause of the ascent of vapour and exhalation; and that the lower part of our atmosphere is, by that means, kept more homogeneous than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matter are kept suspended, at their usual height, merely by this fire. To which I have added something concerning the cause of the winds, and to explain the general phenomena of the weather and barometer.

But as I must now contradict some generally received opinions, it may be thought decent to make some apology for offering my own. But I shall not  
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take up your time in so doing; for if they carry an appearance of truth, I need not make an apology for them; if they appear false, they cannot injure those opinions, which they contradict; and then I have but one apology to make, which is, to acquiesce, and sincerely ask your pardon for having troubled you with them.

It is agreed, that the ascent of vapour and exhalation through the air may be effected two ways; by impulse, and an alteration of their specific gravity.

That vapour does not generally ascend by impulse, may be proved by many familiar experiments, *viz.* Put boiling water into a vessel, and then empty it, and hold the vessel with the aperture downwards: the vapour, which is afterwards expelled from the vessel, must be in a direction downward; but we find, that as soon as it has got but a very little below the rim of the vessel, it has its direction altered, and ascends by the laws of specific gravity. The same thing may be observed in all boiling vessels, where the vapour is emitted in a direction downward; or, in cold weather, when the vapour of a man's breath may be seen, let him breath downward, and the direction of his breath will be presently altered, as in the former case. Since then vapour does ascend without any other impulse than that, which is incident on all bodies ascending by the laws of specific gravity; it is necessary to inquire, how the specific gravity of vapour is altered to cause its ascent. This is generally supposed to be done by filling vesicles of water with rarified air, until the diameter of the vesicle be ten or more times the diameter of a drop  
of

of water, composed of the same constituent particles; and that the vesicle, by this means, becomes specifically lighter than air. But I think, that this cannot be done so easily, as it has been generally imagined; and when done, it will not be sufficient for the purpose; which will appear from the following considerations.

First, the great difficulty in forming those vesicles, especially of the particles of dry bodies carried off by exhalation, and filling them with rarified air, while the exterior air remains condensed. Secondly, That there is not any allowance made for the weight of the included air. Thirdly, The constituent particles of water are but very little (if at all) altered in their specific gravity. Fourthly, That this thin vesicle can never be a sufficient boundary between the exterior condensed air and the interior air, so exceedingly rarified. And therefore as the density of air is proportional to the force compressing it, it follows, that the interior air must be suddenly compressed (and the vesicle with it), until it be of equal density with the exterior air; in which state the vesicle is not more fitted for ascending, than if the same constituent watry particles were formed into a spherical drop.

For here I must beg leave to set aside a false reasoning, which I have found in many approved writers on this subject, who assert, that the specific gravity of bodies is altered by a mere increase of surface, upon their being divided into minute parts. Which is wrong; for the specific gravity of any body is only to be altered by making that body occupy a greater or less proportion of space. It is evident, that upon  
dividing

dividing a body into parts, the surface increases according to the number of parts, but the proportion of space occupied (and consequently the specific gravity) remains the same. Take a cube, whose side is ten inches, the surface is six hundred inches, the space occupied one thousand cubic inches; divide that cube into a thousand cubes; the surface will be increased to six thousand square inches, but the space occupied will remain a thousand cubic inches, and therefore the specific gravity will remain the same: and so on, divide it into as many parts as you please. Indeed bodies by an increase of surface meet a greater resistance in passing through any medium; but, I think, resistance and specific gravity are here carefully to be distinguished: for that resistance, which prevents the sinking of the minute parts of a body, in a fluid specifically lighter, must equally retard their ascent in the same fluid; and therefore can never be the cause of their ascending.

But let us suppose, that this vesicle is formed and filled with rarefied air, and ascending through the atmosphere; it is plain, from another cause, that it cannot remain so during a few seconds of time: for no solar or culinary heat can expand air so as to make it permanent in that state, in a vesicle of vapour ascending through the open atmosphere, for the space of one minute. For it is evident, that metallic bodies, heated many degrees hotter than boiling water, and exceedingly larger than the ascending vesicle, cool in a few seconds, when exposed to the open atmosphere. For instance, mechanic operators, in tempering of small drills, only heat them red hot in the flame of a lamp, and then waving them three



or four times through the air, find them not only cold, but hardened by their sudden cooling, so as to be fit for cutting other parts of the same steel, which has been cooled more gradually. Now the vesicles being exceedingly smaller than the points of such drills, must cool much quicker, and the included air be condensed equal to the circumambient air almost instantaneously; in which state the vesicle is not fitter for ascending than that portion of water was before the vesicle was formed: for though the surface of the water is vastly increased by the form of the vesicle, which may retard the descent of it through the air, yet that form must equally obstruct its ascent; and the specific gravity of the constituent particles remaining the same, I cannot see how the increase of surface can aid its ascent. And here I must observe, that it has not yet been explained, how water can be dilated so, as to occupy eight hundred or a thousand times the space, which it naturally does. For I think the greatest expansion it suffers by boiling (the greatest heat we can give it) is not more than a nineteenth or twentieth part of what it was before.

But supposing these vesicles formed and ascending, there is still a very necessary power wanting, I mean repulsion, to prevent their running into contact (by their natural attraction), and forming larger masses; in which state they would immediately descend. Now let us suppose, that they ascend by impulse; it will be very difficult to explain their motions, which are evident to sense. For if they ascend by impulse, their motion must be continually retarded by their gravity, and the obstructions of the air, until they

arrive at their greatest height, which must always be in proportion to their first velocity; and then they must descend by the laws of specific gravity, as all other bodies do. The velocity with which we generally see vapour ascend, is not sufficient to carry it a few yards in height; and therefore, if there were no other cause of ascent but impulse, the vapours and exhalations must be pretty equably diffused over the surface of the earth, ascending to a small height, and immediately descending again; or if it were possible for them to float at that height, the consequence must be a perpetual fog. How far the great uses of vision would be obstructed or lost in this case, I need not mention; nor need I say, how detrimental it would prove to respiration, since mankind have agreed to call fogs unwholesome. However, when I come to speak of the uses of electrical fire in animal life, I shall say something on this subject.

However necessary an impulsive power may be in the first emission of vapour and exhalation from their masses, I think it evident, from the slowness of their ascent, that it cannot carry them to that height, where clouds are usually formed; much less is it possible, that it should be the cause of their floating there for many days, as we see they do. This can only be done by an alteration of their specific gravity, till they are brought into an æquilibrium with part of the atmosphere where they float.

It now remains to inquire, by what means this may be done; since neither impulse, rarefaction of the air, or any formation of their parts by expansion, (which I know of) seem sufficient for the purpose.

There appears to me but one way of altering the specific gravity of the particles of vapour and exhalation, to render them lighter than air: which is by adding to each particle a sufficient quantity of some fluid, whose elasticity and rarity are exceedingly greater than that of the air. That the fluid or fire of electricity is such, I believe will be easily granted me: but how far it is adapted to this purpose, we must inquire from experiments.

But first, let me not be thought absurd in saying, that the Creator might have thought proper to adapt this fluid to this particular purpose. For the purpose is great; no less than all vegetation and animal life depending on the ascent and descent of vapour and exhalation.

I need not run into a prolix detail of electrical experiments to prove the properties of this fire, which are much better known to you; although I have made some experiments, which, perhaps, have not yet come before you; by which it appears, that all fumes arising from fire, whether blazing or otherwise, and all steams rising from boiling or warm waters, and from all other fluids, and the breath of man, and of all other animals, and all the effluvia thrown off by perspiration, are strongly electrified. But I shall not trouble you with these, till I come to speak of the uses of this fire in animal life. I shall now only mention a few, which are well known, and which are to my purpose. First, That desultory motion, by which it flies off from an electrified body to any number of non-electrics, which are brought within the sphere of its activity and affection, until it be equably diffused through all.

Secondly,

Secondly, That the sphere of its activity is increased by heat. Thirdly, That this fire does not mix with air. Fourthly, That it intimately pervades water, and many other bodies, covering their superficies to a certain distance ; which distance is not in proportion to the bulk of the body electrified, but in proportion to the state of activity of the electrical fluid. Fifthly, This electrical fluid readily joins with any fire, which fumes, or rather with the blaze or fumes of any fire ; but will not mix or fly off with the fire of red hot iron, or any other metal, which does not fume. I have not met with this in any writer on electricity, but I have proved it by experiments.

Now, to shew, that this electrical fire or fluid is the principal cause of the ascent of vapour and exhalation, we need only prove, that it attends all vapour and exhalation, and that in such quantity, as is necessary to render them specifically lighter than the lower part of the atmosphere.

I shall not undertake to determine, by what cause vapour and exhalation are detached from their masses, whether by the solar or culinary fire, or by the vibrations of the electrical fluid rendered more active by those fires ; though I am led to think the latter. But it is evident, that they are emitted in exceeding minute distinct particles, and that these particles must pass through that electrical fluid, which surrounds the surface of the mass ; and that, by that means, they must be equally electrified with the mass ; that is, they must be covered with the electrical fluid to as great a distance from their superficies as the mass is covered ; which must always be in proportion to the state of activity of the electrical fluid. In which  
state,

state, when they have passed the surrounding fluid, they must be repelled by it; and also repel each other; and if each particle of vapour, and its surrounding fluid, occupy a greater space than the same weight of air, they must be fitted to ascend till they come in æquilibrium with the upper and rarer part of the atmosphere; where they must float, until their specific gravity is altered. As it is very difficult to assign the magnitude of each particle of vapour and exhalation, and that of the surrounding fluid; and to shew, that both, taken together, occupy a greater portion of space than the same weight of air; we can only apply to experiment, to shew, that it is possible that it may be so; and that will shew, that in all probability it is so; since it is evident, that every particle must be endued with a portion of this electrical fire or fluid, and that there is not any other sufficient cause assigned for their ascending.

It is evident, that, upon electrifying any light matter, such as down, or the downy parts of feathers, their specific gravity is much lessened; and that, by holding another electrified body under them, they may be driven upwards at pleasure. It is also evident, from experiment, that the more you divide the parts of such bodies, the more of their specific gravity they will loose by being electrified; and by dividing them into very minute parts, I have found, that they ascended to a considerable height after they were electrified. From whence I think it highly probable, that the exceeding small particles of vapour and exhalation may be, and are, sufficiently electrified to render them specifically lighter than the lower air; and that they do ascend by that means.

And

And that they will ascend proportionally higher, as the surrounding fluid is proportionally greater than the particle, which is carried up.

It may be now thought necessary to shew what becomes of our vapour and exhalation above; or how they are to come down again. But I must here beg your patience, while I endeavour to shew, that the ascent and descent of vapour and exhalation, attended by this fire, is the principal cause of all our winds. In doing of which, I hope to bring down the vapour and exhalations, and to explain the general phenomena of the weather and barometer.

It being admitted, that wind is only air put into motion, many have been the conjectures how that motion is caused. Among which, the motion of the earth, and the air's being rarefied by the sun, seem to stand first. The trade winds being most regular, and occupying a considerable part of the globe, it has been thought proper first to account for them, from the afore-mentioned causes.

But I think, that these causes by themselves are not sufficient for the motion of those winds, and much less so for the irregular motion of all the other winds. If the apparent motion of the air was occasioned by the diurnal revolution of the earth from west to east (by the air's being left behind), the motion must be found more regular, and very different from what it is; for in that case the greatest motion must be at the equator, and from thence lessen by regular degrees to the poles; and must be continued always equally one way, both day and night, and at all seasons. But we find quite the contrary; the most gentle gales blowing at the equator

tor and between the tropics pretty steadily, one way all day long, and dying away at night; while high winds and storms, blowing all manner of ways, are found in the higher latitudes. I shall not detain you to shew, that the sun's rarefying air cannot simply be the cause of all the regular and irregular motions, which we find in the atmosphere; but I shall proceed to what, I think, is the cause, *viz.* the ascent and descent of vapour and exhalation, attended by the electrical fire, or fluid.

You are well acquainted with the calculations (by sundry authors) of the quantity of vapours raised in a day; but I will venture to say, that all those, which I have met with, fall exceedingly short of the quantity raised in the torrid zone. Now, all this vapour and exhalation, being buoy'd up by the electrical fire, must add a column to the air (though of a different matter) at least a thousand times greater than the vapour and exhalation taken up; which column must necessarily force the adjacent part of the incumbent air upwards; and must as necessarily be reacted upon by the incumbent air, to restore the æquilibrium of the whole air. And as it cannot be readily forced down again, it must float off, at that altitude, toward those parts, where little or no addition has been made to the atmosphere; and by that means must propel the air on the horizontal level with it, and that below it, as it is itself propelled by the weight of the incumbent air. And that motion must be from the equator (where the greatest quantity of vapour, &c. is raised), toward the poles, and partly to the west; as the column of vapour is always rising from east to west, as the earth turns toward

ward the sun. For here we must confess, that the sun is the great agent in detaching vapour and exhalation from their masses; whether he acts immediately by himself, or by his rendering the electric fire more active in its vibrations: but their subsequent ascent I attribute intirely to their being render'd specifically lighter than the lower air, by their conjunction with this electrical fire. The fire, which surrounds the vapour, beginning to condense, and the vapour to subside, in passing the tropics, becomes a greater pressure on the air beneath, and by that means forces some part back into the tropics, in the place of that air protruded by the ascent of the vapour, &c. and the remainder in a direction toward the poles. The common rotation of the air in coming in below, to supply the place of that part carried up by any fire, may explain this motion. To shew, how this motion must tend to the west, we must consider, that the column of air, raised by the ascending vapour, &c. is at its greatest altitude to the east; and therefore must press that air to the westward, which is continually protruded by the vapours, &c. beginning to ascend from east to west; and the compressed air at the tropics must tend to the westward, till their forces meeting make the motion intirely to the west. The air itself being rarefied, and carried up by the reflection of the intense heat of the sun (for heat, in flying off from all bodies, does ascend through the air with great velocity), may be a considerable additional cause of these trade winds; but never can be the sole cause of all the erratic winds. As I am not sufficiently inform'd of the particular formation of the globe, to account



for all the irregular winds within the tropics, I shall only venture to say, that, where such happen, it must be by means of some tracts of land, which rise to a greater height above the horizontal level, than vapours generally do; by which the motion of the vapours is stopped, and the vapour accumulated by succeeding vapour, and the air, upon which they float, is of consequence pressed into a new direction. And from hence may also be explained the cause of the rains, particularly so called in the sea language.

I must now consider what becomes of the vapour, &c. floating from over the tropics toward the poles; which being less affected by the heat of the sun, reflected from the surface of the globe, the surrounding electrical fire begins to condense more and more as it moves toward the poles, and the vapours of course to descend; and that part most, which is most remote from, or is farthest left behind by the sun; and of consequence the higher column of air must tend that way to restore the equilibrium; which motion, at this side the equator, must be to the north-east; and as the vapour, &c. fall again to the earth, the motion must be more to the east. From whence our south-west and westerly winds, which blow a considerable part of the year.

But as this system is too regular to account for the phenomena of the erratic winds, we must consider whence they arise. I have before observed, that tracts of land rising into the atmosphere will stop the regular motion of the vapour, &c. and that the vapour being accumulated by succeeding vapour, the subjacent air must be pressed into new directions. Now this cause added to the daily dilatation of the  
electrical

electrical fire, and the contraction at night, and the coalition of the vapours, to occasion their total descent, will be sufficient to produce a very great variety of winds on this side the tropic. But if I were to set down all that has occurred upon this subject, about upper currents, under currents, and intermediate transverse currents, &c. occasioned by the various motions of the atmosphere, to restore an equilibrium, and to produce my proofs, I must send you a book, instead of a letter. But I write not to common minds.

It now remains to shew, how the general phenomena of the weather and barometer arise from this system. First, Why it generally rains in winter, while the wind is south, south-west, and westerly. Secondly, Why north-west winds are generally attended by showers in the beginning, and become more dry, as they are of longer continuance. Thirdly, Why north and north-east winds are generally dry. Fourthly, Why the east wind continues dry and dark for a considerable time together. Fifthly, Why squalls precede heavy and distinct showers; and why a calm ensues for some little time after they are pass'd. Sixthly, Why storms and high winds seldom happen in a serene sky without clouds. Seventhly, Why the vapours, in warm seasons, coalesce to form those distinct dense clouds, which produce thunder and heavy showers. Eighthly, Why the barometer falls lowest in long continued rains, attended by winds; and why it rises highest in long continued fair weather; and why the intermediate changes happen. Ninthly, Of land-breezes and sea-breezes, and water-spouts.

First, The vapours passing the tropics into colder regions, have their surrounding fire condensed by degrees; which must increase their specific gravity, and lessen their repulsive power; by which means they must both descend, and approach each other, till at last they form dense visible clouds; and these clouds are also accumulated by other succeeding vapours, of like specific gravity, till they form clouds, which are often several hundred yards in depth; which I have often seen, in passing through them up the sides of very high mountains. In clouds of such depth, I think, the coalition of their particles, to form drops, may arise from their motion, and the order of specific gravity. For the air being a great deal denser, and of greater specific gravity, at the bottom of such a cloud than it is at the top; and the particles of vapour, with their fire, being pretty near an equal specific gravity; those particles, which are below, must necessarily be forced upwards, and those above must as necessarily descend, till they all meet at that altitude, whereat their specific gravity would make them float, if they were not prevented merely by their repulsive power: but this repulsive power must be greatly acted upon, if not quite overcome, in some places by this pressure of the air and of the vapours. But supposing the particles brought into a very near approach by this pressure, it will be scarcely possible for them, upon any motion of the clouds, to keep their stated distances without impinging upon one another. And whenever this happens, it is easy to shew how rain is formed. For where-ever two or more particles are forced to approach within their repulsive powers, they will run into contact and  
sphericity.

sphericity (if fluid) by their attractive power, and by that means become covered with the electrical fluid to a greater distance from their superficies, than the other particles are, or than they were before their conjunction (as I have shewn in my letter concerning the cause of thunder); that is, will be more electrified: and consequently (as appears by electrical experiments) the excess of electrical fluid will run off among the other particles; by which means the enlarged particles have their specific gravity increased, and are enabled to descend to a lower region of the air. And the more particles they impinge upon, in their descent, the more their specific gravity and velocity be increased; and the more their velocity is increased, the more particles will they imping upon, till they fall from the clouds in drops; whose bigness will be according to the depth and density of the cloud they have passed through.

It may be necessary to shew, why I assert, that the increase of velocity in the enlarged particle will make it impinge upon a greater number of the smaller particles, in its passage through them. And this will appear by electrifying a feather, which may be driven upwards, by the electric body, with a certain velocity; but if you add a much greater velocity to the electric body, it will overtake the feather, and get within their repulsive powers; and the feather will stick to it by their attractive powers. And the less distance the feather keeps above the electric body, the less increase of velocity is necessary to make them join. You will pardon my prolixity in this particular, because, I think, much depends upon it in the formation of rain.

Having shewn how I think vapour may coalesce into rain, I shall now endeavour to explain the phenomena of the weather, &c.

First, Why our south, south-west, and westerly winds are wet in winter. I have before shewn, that the wind, and the vapour with it, must tend from the tropic toward the pole, and how it may be varied to the north-east and to the east. In the cold winter season the vapour, in this course, must coalesce more and more, as it comes into the colder regions, and at last come down in mists and rain; and the more the column of air is decreased by the fall of such mists and rain, the more room is there for succeeding vapours to be pressed into the same course, and to fall in like manner; and so on; which may occasion the continued course of these winds, and wet, which generally attends our winters. But sometimes, at the fall of these rains, the wind comes in from the north-west, to restore the balance of the atmosphere, and roll the vapours (still floating in the air), by altering their motion, into heavy dense clouds, which, upon their sudden coalition, let fall heavy showers. But by that means the remaining particles of vapour in the cloud have a greater quantity of electric fluid distributed among them; which enables them to ascend, and form what the sailors call a hard dry sky; and as they approach farther into a warmer climate, which increases the power of the electrical fluid, they rise, dissipate, and vanish out of sight; and this is the general consequence of north-west winds. As to the north and north-east winds, whatever vapour they bring with them, has its repulsive and ascending power (that is the electric fluid) so increased

creased by approaching into warmer and warmer climates, that it scarce ever comes down in rain, except from the effects of some upper current of the air.

It sometimes happens, while the wind blows very gently at east, that the vapours coalesce at a considerable height in the atmosphere, so as to form one even uniform cloud, sufficient to shut out the bright shining light of the sun, which is therefore called a dark sky: Which cloud being uniform, and of no great thickness, and carried on by a very gentle motion, and that neither approaching to a warmer or a colder climate, and seldom descending so low as to be disturbed by the tops of mountains, the vapours may keep the order of their specific gravity for a considerable time; as they generally do, till some cross or opposite wind forces them to separate into denser clouds, and leave apertures for the sun to shine through; and this is generally the case for a day or two before any wet descends.

Why squalls precede heavy distinct showers, and a calm ensues for some little time after they are passed, is pretty evident; for the descending rain, attended by the electrical fluid, forces the subjacent air out of its place, and that which is driven forward must add a proportional velocity to the motion of the wind that way, and that which is driven backward must also obstruct the motion of the wind advancing after the cloud. The wind preceding the cloud is also pressed off obliquely toward each extreme of the shower. Which may be of good use for sailors to know and observe; for if they sail upon a wind from the center of the shower toward the extremity, they may safely venture to keep their luff,  
but

but if they fail from the extremity toward the centre, if they luff up, they will be taken aback, and run the hazard of being difmasted: and this I always found true in practice.

Why high winds are feldom found in a ferene fky without clouds, is alfo pretty evident; for clouds occasion thefe high winds in a double manner; firft, when they are large and united, and upon the defcent, as they occupy a great fpace in the atmofphere, they muft prefs the fubjacent air into a great velocity; and fecondly, when they coalefce, and come down in heavy rains, they make room for the air to flow in with violence, to reftore the equilibrium of the whole air.

To know how the vapours coalefce in warm feafons, to form thofe diftinct denfe clouds, which produce thunder and heavy fhowers, we muft firft confider the ftate of the vapour in its afcent, which muft be ftrongly endued with the electrical fire to enable it to afcend to the great height it then does in the atmofphere: Secondly, The great quantity carried up, and kept afloat at that great height, during a feries of hot dry weather. The quantity may be proved from the height of the mercury in the barometer (as I fhall fhew prefently), and the great height and diffipation of the vapour; from the ferene appearance of the fky; and alfo by the great height, where even thunder-clouds are formed in its defcent; which may be eafily meafured by the length of time between the light and noife of thunder. In this ftate the upper air is in a quietude, and whatever part of this vapour begins to coalefce and fubfide firft, will carry down with it part of the fubja-  
cent

cent vapour, and make room at the top for other vapours to flow in from all sides, which, by their meeting, may also coalesce and subside to form this central descending cloud, and so also of like succeeding vapours. And thus, I think, clouds of any density may be formed in a quiet air (and thunder generally happens in calm weather), till the density is too great for the buoyant electrical fire to sustain the weight, which flying off, in part, from the largest drops, lets them fall almost in spouts.

Why the mercury falls and rises in the barometer, will appear from what I have been saying; for in long continued fair dry weather, there is a great quantity of vapour raised, and kept afloat in the air; which must necessarily add a column to the air at least a thousand times greater than the vapour taken up; and often much greater than that; which must as necessarily increase the weight of the air, and make the mercury rise proportional to the additional column; and when rains descend, they subtract a column from the air, near a thousand times as great as the water fallen, which must necessarily lessen the weight of the air, and make the mercury fall proportional to such subtraction. As to the winds, they affect not the mercury, but as they are generally, in these climates, a consequence of descending clouds and rains, blowing into those parts, where the column of air has been lessened, and while they blow, the mercury will continue low in the barometer, till the equilibrium of the atmosphere be restored. But, I believe, if it were inquired into, whether the mercury descends, when the wind begins to blow every morning between the tropics, and the



vapours to ascend, that it will be found, that the mercury rather ascends.

There is a phænomenon, which has puzzled many; that is, Why the mercury falls before the rain falls? This may be explained by the column of air's being lessened gradually, by the condensation of the electrical fluid from the first descent of the vapours till they fall in rain.

I shall now say something of land-breezes and sea-breezes, a phænomenon, which sometimes happens in fair settled weather, that the wind blows out from the land at night, and in from the sea at day-time. The land-breeze is occasioned by the descent of the clouds, and the particular formation of the land; for if the land rise into a hilly country from the sea, when the clouds and vapours descend at night, which they often do by the electrical fluids being condensed, they must press the air down the land toward the sea in their fall; as may appear from the smoke of any fire running down the side of a hill, in the evening of a damp day, when the clouds are upon the descent. And the sea-breeze is occasioned by the clouds ascending in the day-time, which must impel the incumbent air upwards, and make room for the sea-breeze to flow in: but, beside the mere ascent of clouds, there is an exceeding greater quantity of vapour raised from the land than from the sea. For the same extent of land has an exceeding greater surface than the same extent of sea; which may appear from the various forms of vegetables and animals, &c. and the greater the surface, the greater will the evaporation be. Beside, the more irregular these surfaces are, the greater will be  
the

the reflection and refraction of the sun's beams, which will increase their power. And it is also necessary, that the evaporation should be much greater from vegetable and animal fluids, than from fluids in a quiescent state, to carry on a circulation for the great work of nutrition : but of this when I come to speak of vegetation and animal life. Now, the ascent of these vapours must beget a circulation of the air inward from the sea ; in the same manner as the ascent of vapours from any fire brings in the air below to that fire.

As to water-spouts, I have found them oddly described by the learned, as being great columns of water sucked up from the sea by the clouds. But I never saw any such ; nor could I find, upon inquiry, from many honest men, who have sailed almost all our known seas, that they ever met any such ; and therefore I do not believe that there are any such. There is indeed an appearance something like their description, which may have given rise to their conjectures ; but this is no more than a very heavy shower from a very dense cloud, which is drawn into a conical form, and a very narrow compass at bottom, before it arrives at the sea ; which it dashes with great violence in its fall. It may be worth the while to inquire how the shower comes into this conic form ; which I think is caused by the general attraction. For while the vapours floated in the air, being of like specific gravity, they were equally attracted by the air as by one another ; but when their specific gravity was greatly increased by their being formed into larger drops, and letting go great part of that electrical fluid, which buoy'd them up,

and the greatest quantity of water falling in the center of the shower, the extremes may be greatly attracted to the center, in their passage downward.

There is one objection, which may be made to all I have been saying of the winds and weather; that is, the great distance of the tropic, and that the wind must be a great time in coming to us from thence. But this objection will lessen, when the velocity of the wind is considered, which in a fresh gale will move a degree in two hours; which will soon bring it from the tropic to us. And this velocity, and a much greater, I proved by a machine, which I invented, and made, in the year 1733, to go upon wheels, by the force of wind. For I have been carried in it at the rate of more than a degree in two hours, when the wind was upon the beam; that is, when I could sail forward or backward upon the same tract, with equal facility; so that my motion going before the wind, must have been much greater, which I then neglected to estimate.

And now, Gentlemen, I fear two things; that I have said too much, and that I have said too little. For if this letter appears with the same face of truth to you as it does to me, I have omitted many things, for fear of being too prolix, which I ought to have added. But, if I have written without a foundation in truth, I must, long before you come to this part of my letter, appear prolix and impertinent; and therefore it may be time to conclude. Before I do which, I will assert one thing; that is, that if any thing in this letter is found worth your acceptance, it is intirely my own; for I have not borrowed a single hint. No more I did in my letter concerning the  
cause

cause of thunder. For when I wrote that letter (which was on the 20th of September 1751), I did not know, that any one had the least conjecture, that the electrical fire was the cause of thunder. And I told many gentlemen in this kingdom, in the beginning of the year 1751, that I thought I had discovered the real cause of thunder. Indeed, immediately after my sending that letter to you, on the 18th of June 1752, I met some accounts in our news-papers, that iron bars, some how or other set up, during the time of thunder, were found to be electrified; but from that time to this I know nothing particular of those, or any other experiments on thunder-clouds. I waited the publishing of your volume of Transactions, for the year 1752, to inform myself of these matters; but I do not find, that it is yet come to our kingdom.

As I am not a plagiarist, so I would not willingly be thought one; nor should I venture to trouble you with what I thought you may have better from other hands; but as the matter of this letter appears new to me, so I venture to lay it before you, without any other motive than to add my mite in the investigating truth. I am,

Gentlemen,

Your most humble

and most obedient servant,

Hen. Eeles.

My

My fear of trespassing upon your patience had almost made me forget to explain, how far the wind assists vapours to rise in greater quantities, and in less time, than they would in a quiet air. And this omission would not be excusable, because both the learned and unlearned have observed, that a greater quantity of vapour is carried off in dry windy weather, than when the weather is calm. When vapours are emitted in great plenty from the surface of fluids, such as from a pot before it begins to boil, or from the surface of a bowl of hot punch, &c. it often happens that they crowd each other in such manner, that the surrounding electrical fluid of each particle is compressed, and cannot occupy that portion of space, which is necessary to make it buoy up the particle: and thus the particles float in a steam just above the surface of the liquor, and they are prevented from falling back into the liquor by the electrical fluid on the surface of the liquor; in which state none but the uppermost particles can ascend; which they do by the elasticity of the electrical fluid's forcing them upwards, till they have room for the electrical fluid thoroughly to expand itself, so as to be able to buoy up the inclosed particle of vapour.

In this state the assistance of the wind will be easily understood; for the wind blowing the vapours from the surface of the liquor, dissipates them, and gives them room to ascend, and clears the surface of the liquor, to make room for the succeeding vapours to ascend, and so on; which in this case will greatly aid the ascent of vapour. And thus far I think that the wind assists in the ascent of vapour, but no farther.

I have

I have mentioned hot liquors only, because the steam is more visible in them: not but the same case often happens in evaporation, where we cannot so easily perceive it.

L E T T E R III.

*To the Reverend Thomas Birch, D.D.  
Secret. R. S.*

S I R, Liffmore (in Ireland), March 31, 1755.

Read April 24, 1755. **I** Received your obliging letter of the 9th instant, of which I am highly sensible, and shall always be subservient to your commands.

You do me the honour to tell me, that the Royal Society desire to know the experiments, by which I found all ascending vapours and exhalations to be electrified. At first I only supposed they must be so, according to the reasonings in my letter; but upon trial, with a very simple apparatus, I had the pleasure to convince myself that they were so.

I extended a fine string of silk eight feet horizontally, and from the middle suspended two pieces of such down as grows upon our turf-bogs, by two pieces of fine silk, about twelve inches each in length; and then, by rubbing a piece of sealing-wax on my waist-coat, over my side, I electrified the pieces of down; and then brought sundry burning things under them, so as to let the smoke pass in great plenty through and about them, to try whether the electric fluid would run off with the smoke; but  
I had

I had the pleasure to see that the down was but a little affected by the passage of the smoke, and still remained electrified. I then brought sundry steams from the spout of a boiling tea-kettle, and otherwise, in the same manner, and still found, that the down remained electrified. I then breathed on them in great plenty, but found that the down still remained electrified. I then joined the palms of my hands together, with the fingers extended perpendicularly under the down, which still remained electrified; although the subtile effluvia, thrown off by perspiration, passed in great plenty through the down; as may appear by holding one or both the hands in the same manner under any light matter floating in the air, which will be driven upwards thereby, with as great velocity as an electrified feather is by any electrified body held under it. In short, I tried all the vapours and exhalations I could think of, in the same manner, and with the same success.

I then warmed a wine-glass, and with the skirt of my coat held inside and outside the glass between my fingers and thumb: I rubbed the glass briskly about, and electrified the down, and found all experiments answer in the same manner as they did with the wax. I mention this particular, because some writers on electricity have said, that there were two kinds of electrical fire, the one resinous, and the other vitreous; because light bodies electrified by glass are attracted by electrified wax, &c. and those electrified by resins are attracted by glass. But I think these different effects must arise from some differing qualities in the resin and glass, which have power to actuate this fire differently. For if there  
were

were really two distinct species of this fire, opposite in their nature; the afore-mentioned experiments would have a very different consequence from what appears. For if the vapours were impregnated by the vitreous fire, they must absorb, or some way disturb, the resinous fire, which electrifies the down, and so *vice versa*: but we find, that the same vapour, with its electric fire, passes through the electrified down in the same manner, whether it be electrified by glass or resin. But I will not detain you on this subject.

The electricity remaining in the electrified down after these experiments made it appear, that the smoke and steams must be either electrics, or non-electrics electrified. It was easy to suppose them non-electrics, as they arise from non-electric bodies; and the more, because the highest electrics by a discontinuity and comminution of their parts (long before they come to be as minute as the particles of ascending vapour), become non-electrics, or conductors of electricity. For glass, resin, wax, &c. all become non-electric, even in fusion. But to try whether the steams, &c. were non-electrics, I only bedew'd the wax and glass with my breath, steams, &c. from my hand to the end of the wax and glass; and then touching the electrified down with the end of the wax or glass, I found, that the electrical fire immediately passed from the down into my hand, through the steams, &c. which rested upon the wax and glass. Which, I think, sufficiently proves the steams, &c. to be non-electric; and I think, that it as plainly appears, that they are all electrified while ascending, because the electrical fire in the down



does not join with them in their passage through it ; which otherwise it would do with them, or any non-electric not electrified.

I made some other experiments to this purpose ; but these principally persuaded me to say, that all ascending vapours and exhalations were electrified. But how far they will weigh with the Royal Society, I must leave to their better judgment. I am,

S I R,

Your most humble and

most obedient servant,

Hen. Eeles.

*P. S.* I electrified the down between each experiment.

XXVI. *Remarks upon a petrified Echinus of a singular kind, shewn to the Royal Society, April 24, 1755, by the Reverend Richard Pococke, LL. D. Archdeacon of Dublin, and F. R. S. found on Bunnan's-Land, in the Parish of Bovingdon in Hertfordshire, which is a Clay, and supposed to have been brought with the Chalk, dug out of a Pit in the Field. By James Parsons, M. D. and F. R. S.*

July 9, 1755.

Read April 17,  
1755.

THE round echinities are for the most part found in chalk-pits, and they are in general, when recent, the most tender in their shells; so that the chalk is the most favourable bed for them to be preserved in long enough to be petrified; whereas in other kinds of matter these would be mouldered and destroyed before the petrification could commence; and it is very singular, that almost all those in the chalk are filled with flint, or partly chalk and partly flint, and sometimes with crystal. Now, as all flints and agates are nothing less than crystal debased by earth, and as it is in beds of chalk that these, as well as multitudes of large stones, are found, one would be almost induced to believe, that chalk degenerated

into flint ; or, in other words, that flint was produced by chalk originally. And indeed I have many specimens myself, that seem to prove it; in some of which they seem to shew the gradual change from the one to the other, not at all like a sudden apposition of chalk to flint.

Other kinds of echinites, such as the *Echini cordati*, or heart-shaped echinite, the *pileati* or conic, the *galeati* or helmet-shaped, with several other kinds, are often formed of other species of stony particles.

The fossil before you, being one of the oval kind, with large papillæ, is the *Echinometra digitata secundæ rotunda vel cidaris Mauri* of *Rumphius*, which, with the other oval echinites, are very rarely found but in chalk : and it is remarkable, that whether they are filled with chalk, flint, or chrystal, their shells break with a selenitical appearance, just as the lapides Judaici, and all other species of echinites found in chalk-pits, do.

XXVII. *Two Letters concerning Toxicodendron.*

## L E T T E R I.

*From the Abbé Mazeas, F. R. S. to the Rev. Stephen Hales, D. D. F. R. S. Translated from the French, by James Parsons, M. D. and F. R. S.*

S I R,

Paris, Aug. 16, 1754.

Read Dec. 19,  
1754.

**I**T is not long since (while I was making some experiments upon the painted cloths made in Europe, in order, if possible, to bring them to greater perfection) I received a letter upon the same subject from the Abbé Sauvages, of the Royal Society of Montpellier. In this letter he communicated a discovery of a plant, the juice of which adheres, without the least acrimony, to a cloth, with more force than any other known preparation. The colour is black, and the plant, which produces it, is the *Toxicodendron Carolinianum foliis pinnatis, floribus minimis herbaceis*.

I was then upon the point of going to St. Germain, where the Duke D'Ayen has a bontanical garden, which is the most complete in the kingdom. My first care was to confirm the Abbé Sauvages's discovery; they shewed me the plant mentioned, which, they said, was a native tree of Carolina, but which

was

was not yet more than two feet high. This tree is remarkable for its leaves, which are continued like wings the whole length of the twigs. I pulled off one of the leaves, the juice of which produced a brownish colour upon my ruffle, but did not change black in less than two or three hours.

I had a mind to examine all the plants of the same class. Near this was the *Toxicodendron triphyllum folio sinuato pubescente*. T. 611. *Hederæ trifoliæ Canadensis affinis planta peregrina, arbor venenata quorundam*. H. R. Par. 84. *Arbor trifolia venenata Virginiana folio hirsuto*. Raii. hist. 1799. This plant, which was no less a tree than the foregoing, is not as yet above three feet high; its leaves are hairy; their pedicles, ribs, and fibres, are red; a leaf being pulled off, a milky juice issued from the pedicle, which being put upon linen, became a finer black than the former, in less than half an hour.

In this botanical garden I saw another species of *Toxicodendron*: this, however, was but a shrub, which appeared to me to be at its full growth. It is the *Toxicodendron triphyllum glabrum*. T. 611. *Hedera trifolia Canadensis* Corn. 96. *vitis sylvestris trifolia*. Park. Theat. 1556. This plant is remarkable for having an infinite number of black points scattered upon the surface of its leaves, which seem'd to me to be a juice extravasated through the punctures of insects. A leaf being pulled off, a milky juice flowed out, which, the instant it was exposed to the sun, became the finest and deepest black I had ever seen.

I doubt not, but that if these two trees of Carolina were of their proper height, they would produce as fine a colour as this last shrub. However, I put the linen marked with the three black spots into a boil of soap, and it came out without the least diminution of the colour of the spots. When this linen was dried, I threw it into a strong lye of the ashes of green wood; and it, in like manner, came out without the least alteration of the three shades of the spots, produced by the three plants mentioned.

I took a handful of the leaves of the *Toxicodendron glabrum*, to try if it might be of use in dying; and made a very strong decoction of it; and while it was boiling I dipped linen in it: it was tinged green, but, besides its not being a good green, the whole surface was unequally coloured; for I observed several places took a fine black: whence I concluded, that the resinous juice of the internal parts of the plant was the only part capable of producing the desired effect. I was confirmed in this notion, after having let my decoction settle; it first let fall a black resinous juice in small quantity, like the opium of our shops: then a large quantity of a white sediment like a salt, which was quite tasteless upon the tongue. In short, the water appeared greenish above, and blackish towards the bottom of the vessel.

I should have been glad to try some experiments upon the Roots of this plant; but, as there was only one in the garden, I was afraid of injuring it. Perhaps the fruit or seeds might produce some kind of dye,

dye. I do not doubt but that, in making incisions in the bark, one might even obtain a juice which might be turned to some use: for the blacks of our painted cloths, which are preparations of iron with nut-galls, after a certain number of washings, are quite spoiled, and only leave a rusty colour behind. But it is not so with the *Toxicodendron foliis pinnatis*, since the Abbé Sauvages assures me, in his letter, that it is five years since his linen, marked with the juice of this plant, has retained the black spots, notwithstanding the great number of washings in lye it has gone through.

I beg, Sir, you will present my respects to all my good friends, and be assured of the sincere and inviolable attachment, with which I shall, during life, remain,

Your most humble

and obedient servant,

W. Mazeas.

## L E T T E R. II.

*From Mr. Philip Miller, F. R. S. to the  
Reverend Thomas Birch, D. D. Secret.  
R. S.*

S I R,

Read May 8, <sup>1755.</sup> **I**N the Abbé Mazeas's letter, which was read before the Royal Society, on the 19th of December last, he mentions, that while he was making some experiments upon the painted cloths made in Europe, in order, if possible, to bring them to greater perfection, he received a letter upon the same subject from the Abbé de Sauvages, of the Royal Society of Montpellier, in which is communicated a discovery of a plant, the juice of which adheres, without the least acrimony, to a cloth, with more force than any other known preparation. The colour is black, and the plant, which produces it, is the *Toxicodendron Carolinianum foliis pinnatis, floribus minimis herbaceis*.

The Abbé Mazeas afterwards mentions some trials which he made with the juice of this plant, as also those of two other species of toxicodendron, which were growing in the garden of Duke D'Ayen at St Germain: by which he observed, that the juice of the other two species stained his ruffles of a finer black, and in much less time than that of the first-mentioned; which he supposes may have been occasioned by the quantity of the juice, which flowed from the two last, as the plants were much stronger,



and he was less cautious of wounding them; for he says, the *Carolina Toxicodendron* was so small, that he could only take off a single leaf; otherwise he believes the stains would have been equal.

As the use of this dye is at present but little known in Europe, this may appear as a new discovery; but whoever will give themselves the trouble to turn to the books, in which this plant is described, will find, that this *American Toxicodendron* is the same species of plant, from which the inhabitants of Japan procure the varnish, with which they stain all their utensils; and the Calicuts are also painted with the juice of this shrub.

That this communication of the Abbé Mazeas may not appear in the Transactions of the Royal Society as a new discovery, I shall beg leave to mention a brief account of what has been written upon this subject.

Doctor Kæmpfer, in his *Fasciculus Amœnitatum exoticarum*, has given a figure and description of this plant, which are so accurate, as to leave no doubt of its being the same plant as the *Carolina Toxicodendron*. His book was printed at Lemgow, in 1712. His title of the plant is *Arbor vernacifera legitima, folio pinnato juglandis, fructu racemoso cicris facie*. And by the inhabitants of Japan it is called *Sitz* vel *Sitz dſſu*, as also *Urus* seu *Urus no ki*. In the same book there is a figure and description of the wild varnish-tree, which he calls, *Arbor vernacifera spuria sylvestris angustifolia*; and the inhabitants, *Fasi no ki*; but the varnish, which comes from this tree, is of little esteem.

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Here I must beg leave to mention, that the seeds, which were sent to the Royal Society some years ago, for those of the true varnish-tree, by the Jesuits at China, prove to be of this wild sort; and the account, which those reverend fathers sent of the manner, in which the varnish is procured, being so very different from that, which is mentioned by Doctor Kæmpfer, I shall here transcribe it.

He says, they first slit the bark of the branches of the shrub, in different places, with a knife: from these wounds there flows out a white clammy juice, which soon turns black when exposed to the air: the same juice, he says, is contained in the leaves and stalks of the plant. This juice has no other tasteable quality but that of heating without turning sour, but it is dangerous to handle, being of a poisonous nature. When they make these incisions in the branches of the trees, they place wooden vessels under them, to receive the juice as it drops from the wounds; and when these become dry, and will afford no more juice, they make fresh wounds in the stems of the shrubs, near their roots, so that all the juice is drawn out of them: then they cut down the shrubs to the ground, and from their roots new stems arise, which in three years will be fit to tap again.

This native varnish, he says, scarcely wants any preparation; but if any dirt should happen to mix with it, the Japanese strain it through a coarse gauze, to cleanse it; then put it into wooden vessels, covering it with a little of the oil called *Toi*, and stretching a skin over it to prevent its evaporating. Then, being thus put up, it is carried all over China and Ja-

pan for sale. The varnish, he says, expires a poisonous vapour, which occasions great pains in the head, and causes the lips of those who handle it to swell: upon which account the artificers, when they use it, are obliged to tie a handkerchief over their nose and mouth, to prevent these effects.

The shrub is chiefly cultivated in the provinces of Tfi, Kocko, and Figo: and the best varnish in the world, he says, is produced about the city Jassino: but there are many other sorts of varnish, which are collected in Siam, Corsama, and other provinces, which are much inferior in their quality to this, and are produced by different plants: but one of the best among those, he says, is produced from the Anacardium, or Cashew-nut-tree. This is procured by perforating the bodies of the trees, and placing an hollow tube into the hole, under which is put a wooden vessel, to receive the liquor, as it flows through the tube; and when they have obtained as much of the juice as will flow out, they stop the holes made in the trees. This juice is white when it proceeds from the wounds, but changes black when exposed to the air.

This varnish is used, without any mixture, for staining black; but the Chinese mix native cinnabar, or a red kind of earth, with it, to make a different colour.

The plant, which the Abbé de Sauvages mentions, is also figured and described by Doctor Dillenius, in the *Hortus Elthamensis*, p. 390. by the Title of *Toxicodendron foliis alatis, fructu rhomboide*, where he also quotes the description from Doctor Kämpfer, with the account, which is above-mentioned; and he  
has

has added all the synonyms from the different authors, who have mentioned the plant, and makes no doubt of its being the same with that of Japan, which, he says, should not seem strange, that a varnish-tree should be found in America, near the same latitude with Japan; since the Genseng, the Bignonia, commonly called Catalpa, with many other plants, are found to be natives of both these countries. And he questions, if the Tea-tree might not be discovered in America, if persons of skill were there to search for it. And he is surpris'd, that the inhabitants of the English colonies in America have not attempted to procure the varnish, whereby a considerable profit may arise to them, as the plant grows naturally in so great plenty there.

Mr. Catesby, in his *Natural History of Carolina*, vol. I. p. 40. has given a very good figure and description of this plant: he calls it *Toxicodendron foliis alatis, fructu purpureo pyriformi sparso*. And, he says, the inhabitants of Carolina and the Bahama islands call it, Poison-tree, and Poison-ash, as the other two sorts of *Toxicodendron* are called Poison-oak in Virginia and New England. Mr. Catesby takes notice, that from the trunk of these trees is distilled a liquid, black as ink, which the inhabitants say is poison; but does not mention its being used there.

There are two accounts of the poisonous quality of this tree, which are printed in the Philosophical Transactions of the Royal Society, Numb. 367. The first was sent by the Honourable Paul Dudley, F. R. S. from New England, and the other was communicated

municated by Doctor William Sherard, F. R. S. By both these accounts it is very plain, that this species of *Toxicodendron* grows naturally in Virginia and New England, in as great plenty as Carolina, where all the species are the most common under-wood, in the lands which have not been cleared.

I shall only beg leave to add, that as these shrubs are so very common in our northern colonies, and the *Anacardium*, or Cashew nut-tree, is also common in our southern colonies of America; it were to be wished, that the inhabitants of both would make some experiments to collect this varnish, which may not only produce much profit to themselves, but also become a national advantage.

I am,

Chelsea, March  
18, 1755.

S I R,

Your most obedient

humble servant,

Philip Miller.

XXVIII. *A Letter to the Right Honourable the Earl of Macclesfield, President of the Royal Society, concerning the Method of constructing a Table for the Probabilities of Life at London, from the Reverend William Brakenridge, D. D. and F. R. S.*

My Lord,

Read April 24,  
1755.

**Y**OUR character in the philosophical world, in the relation you bear to each member of our illustrious Society, makes me presume to offer my thoughts to you, on a very interesting subject, the probability of human life from the bills of mortality. For as it has some difficulty, and requires an accurate examination of many circumstances, I could not possibly send such calculations to any one who understood them better, or could more quickly discover any mistake, or fallacious reasoning; and I hope your usual goodness, and indulgence to every industrious inquirer will excuse me, that I give you this trouble.

The great Dr. Halley, who had a singular faculty of applying his mathematical knowledge to the purposes of life, was the first who particularly attended to this subject. In the year 1692, from the bills of mortality at Breslau, he reduced it into a sort of science; and gave a table of the probabilities of life, that hitherto has been justly esteemed the most exact of any thing of the kind; from which he and others have deduced many propositions, that are highly

highly useful. But a doubt having arisen, whether that could properly, or with any accuracy, be used by us at London, as we are in a different country, and perhaps in a different way of life, I have been at some pains to inquire into this, and satisfy myself about the objections. And I imagine, that I can now shew how that table may be altered, to suit our case with sufficient exactness.

In the London bills of mortality, for the last 30 years, there is always added an account yearly of the number of burials under each age, at the distance of ten years, and of children more particularly under two years, between two and five, and between five and ten; which numbers are curious and useful. And, I believe, though there may sometimes be some inaccuracies and omissions, these numbers are as exactly given as in our case can be expected: and what may be objected, is not so much to the incorrectness of them, but to what arises from our circumstances, that will not allow them to be proper, to shew the probabilities of life in all its periods.

As I am inclined to think, that no table can be formed from them, as they are at present, that will be sufficiently accurate above 20 years of age, I shall make some observations that seem to shew this, before I proceed to what I have proposed. And that we may have a more accurate view of the numbers, and be able more certainly to reason about them, let us take the sums of all the burials under each age, for the last ten years, from 1744 to 1753 inclusive; and then the tenth part of those sums will give, at an average, the burials at each particular age in a year: And they will stand as in the 2d column of the annexed table.

table. Of which the sum for one year is 22867. But if we suppose, that there are 2000 more burials yearly than in the bills, on account of dissenters of all denominations, and those burials that are carried out of town, and not accounted for, as has been mentioned in my letter last year, concerning the number of inhabitants; the whole number of burials will then be 24867 yearly. And we must take the proportional parts of 2000, and add them to the numbers of burials at the different ages, which will give all the burials at each; and the numbers so augmented will stand as in the 3d column. And if we likewise take the numbers of the dead, at the same periods at Breslau, they will be as in the 4th column. The numbers in the first denote the years.

2	8110	8819	202
2—5	1845	2006	
5—10	741	805	
10—20	682	741	
20—30	1904	2070	
30—40	2301	2502	
40—50	2395	2604	
50—60	1881	2045	
60—70	1481	1610	
70—80	1053	1145	
80—90	474	515	
	<hr/>	<hr/>	
Sums	22867	24862	999

Now when we consider the series of numbers, representing the dead at London, it seems plain, that above 20 years of age it is unnatural; and therefore



cannot be a true representation of the probability of life. In the age between 20 and 30 there appears to die near three times the number, that die between 10 and 20. For the numbers are as 2070 to 741; which cannot possibly be from the natural decrements of life; because we generally see, that people between 20 and 30 are nearly as healthy and vigorous, as between 10 and 20; but it must necessarily arise, from the great increase of the number of the people at that age, by the accession of strangers. And then, on the contrary, above the age of 50, the numbers seem to represent the decrements of life slower, contrary to the natural course: For the numbers of the dead after that age continually decrease. From 40 years of age to 50 they are 2604, but in the three following decennial periods they are as 2045, 1610, 1145; that is, in the more healthy ages there are more burials, than in the more infirm and unhealthy; which is not to be accounted for without supposing, that great numbers retire after the age of 50 into the country. For after that time, as infirmities and diseases greatly increase, the burials ought at least not to be fewer, till the number of people is nearly exhausted. And indeed it is evident to common observation, that a great number in advanced age retire from the town; from which it must follow, that the numbers of the dead cannot give a true representation of the probability of life.

If, in the next place, we compare the numbers of the dead, in the several periods at Breslau, with those at London, we shall plainly see, that the former shew the decrements of life in a natural and regular way, and free from the above difficulties and objections.

tions. In the infant state, under 2 years of age, there is one-fifth lost by death; but afterwards, as they gather strength, the deaths are diminished till between 10 and 20; and from that age the mortality gradually increases, till after the age of 40; when the number of the dead continues nearly the same, though the probability of life continually decreases till the age of 80; and then at length, the living being almost all exhausted, the burials are greatly diminished. All which seems to be agreeable to the course of nature; but, contrary to what we see in the London bills, especially after 50 years of age, as we have already observed. However, they both agree in this, that the most healthy age is between 10 and 20, and the infant state under 5 years of age the most uncertain for life.

But indeed it must be acknowledged, that in computing the Breslau table, Dr. Halley had great advantages, which have made it so perfect. He had the number of births given, besides the burials at the different ages, in an inland town, where there is no great concourse of strangers. But with us at London the number of births is not known; because of the number of dissenters of various denominations, both foreigners and natives, of whose baptisms there is no account taken; which makes our bills at present very imperfect. For none are put into our bills but those who are baptized, according to the form of our established church. And therefore there are some thousands omitted, and yet many, perhaps the one-half of them, who are not baptized with us, bury with us; which greatly perplexes our bills. And under this disadvantage it appears very

difficult, to make an accurate computation of the decrements of life through the different ages; though this defect I imagine I shall be able nearly to supply.

But I think, without examining the series of numbers resulting from our bills, if we only consider the circumstances of our great Metropolis, we shall plainly see, that above 20 years of age there can be no depending on the number of our burials, so as to compute the probability of life. For after that age, there is such a multitude of strangers come here, from all places; some to settle for life, others to stay for a time to acquire some fortune or employment, and others only to remain a few years or months, that our burials are always in a fluctuating state. Servants, journey-workmen, and young people, that are to push into life, generally come at that age; some of which come only for two or three years; and then, if they survive, retire again into the country; all which occasions such a great number of accidental burials, that it seems plain, that our bills cannot be so regular as to shew the probability of life above 20 years of age. And therefore, by the way, it may be observed, that the greatest number of people is between 20 and 30; for at that age there is above 160000, that is, above a fifth of the whole; which is twice the number that is between 10 and 20. And thus, I think there are so great objections to our bills, in the ages above 20, that they seem to me to be improper, to make any computation of the value of lives from them. And it were to be wished, that some inland town could be found in England, where there was kept an accurate register of births and burials, with the ages of the deceased, and where  
there

there is no great confluence of strangers; or rather that a dozen or twenty parishes, contiguous to one another in the country, could be found to keep such a register; for that would be of more general use.

There have indeed been some ingenious men who have thought, that our London bills are correct enough to form a table from them, which may better agree with our circumstances, than that which Dr. Halley has given us. And Mr Smart was the first who endeavoured to do something in this way, from our bills only, about 18 years ago. But, in the table made by him, he seems to have been greatly mistaken; for he has made no allowance for the accession of strangers, but considered the numbers of the dead, in all the periods of life, as all come from those born here; whereas it is evident, that the strangers, above 20 years of age, are at least equal to them. And this has brought this paradox into his table, that young people between 12 and 18, at London, are much more healthy than at Breslau, or in any country place in England. For according to him, in the 13th year, 2 die only out of 479; but at Breslau there die 6 out of 634; that is, there is double the number die more at Breslau than at London; which appears impossible. But between 30 and 40, he makes them much more unhealthy than they are; for at 40 he supposes one to die in 29; whereas there does not die above one in 30, all ages taken together, with infants included, as I have shewn in my letter last year. Another ingenious gentleman, having seen this inconsistency, has endeavoured to correct it, by supposing that the number of strangers that come to settle in town, after 25 years  
of

of age, is inconsiderable ; and that above that age, the numbers of burials may be considered, as arising from the natural degrees of mortality ; and then by proportion, increasing the numbers of the living corresponding to all ages below 25 ; so that the table, altered in this manner, is the same with Mr. Smart's above that age. And it must be confessed, that this correction is very proper, and worthy of its author. But still the table is greatly defective, as he has made no allowance for the recess of great numbers, who after they have been a number of years in town, leave it, if they survive ; and of many others who, after the age of 50, retire from business into the country. And which is so very obvious, that our burials are fewer, than by proportion they ought to be after 50 years of age, as I have mentioned above, and by consequence the people appear more healthy after that age ; so that after 70 they seem more healthy than at Breslau. For at 75 there appears from this table to die 4 out of 45, whereas at Breslau there die 10 out of 88.

And that a great number retire from the town, after the age of 50, or before, is farther evident ; if we suppose, even according to this corrected table, that one in 25 die at the age of 50. For then the number of people alive, between 40 and 50, will be greater than 2604 multiplied by 25, or 65100 ; which ought to be exhausted by all the deaths in the subsequent periods. But all the deaths which ought to arise from that number of living, in the following years to 90, according to the bills in the 3d column, is 5315 multiplied by 10, or 53150 ; which is less than the people that were alive between 40 and 50,  
by

by 11950, or more. And therefore above 11000 of those between 40 and 50, must have retired from town.

But now, as our bills are defective, it is next to be considered, what we at London are to do at present, and what method of computation we are to follow? And I imagine it is very obvious what may be done. Our bills may be used so far as 14 or 20 years; for there is certainly no increase of our people till the age of 14; because few young people come to town till they are fit to be apprentices or servants. And between 14 and 20, though many come at that time, yet there is an emigration of a great number from hence to sea, to other countries, the universities, and country academies, that nearly balances the accession of strangers. And then, after 20 years of age, I believe the Breslau bills will be sufficiently correct, to shew the probability of life within and about the city. And if so, a table may be made from both bills that will agree with our case here with sufficient exactness. For I cannot find, that there is any difference in the bills, above the age of 20, that can be depended upon. And I see no reason why our air should not agree, as well with those in advanced life, as that at Breslau, and our people be equally healthy. And this is not mere conjecture; for, as far as I can examine into it, a variety of things seem to confirm it.

Breslau is nearly in the same parallel of latitude with London, and therefore their seasons cannot be very different; and it is a large town, containing above thirty thousand inhabitants, according to Dr. Halley; and so the air, and circumstances of life, cannot

not be so conducive to health as in the open country. And consequently unless all great towns that are less than London, and nearly in the same latitude, are more healthy, there can no reason be given why Breslau should be so. It is true it cannot be demonstrated, that they are equally healthy, to persons above 14 or 20 years of age; but when we see the tables that have been made from our bills, and observe the inconsistency that appears in them, which is owing to the fluctuating state of our city; we see no reason to think otherwise, and we rather seem to have some presumption that it is so. Because, if we look into Mr. Smart's table, we find, that he has made the people more healthy between 12 and 18, and after 70 years of age, than at Breslau; and yet more unhealthy in the ages between these periods; which seems to be near to a contradiction: For why they should be more unhealthy in the intermediate years, is not possible to be accounted for. One would rather think, that if in the extremities of life they were more healthy at London, they could not be less healthy in the middle, and stronger part of it. Or, if we consult another form of this table ingeniously corrected, we see, that people appear still more healthy after 70 than at Breslau; which seems to be inconsistent with their being more unhealthy in younger life. For one would imagine, that the inclemency of any climate or place should affect people more with the infirmities of age, than those that are younger, and yet in their vigour. But if we account for this variation of the table, by supposing, that after the age of 50, many retire from town, as I have mentioned

tioned above, this will destroy the whole hypothesis, upon which the table is formed.

If we suppose that one in 34 die at Breslau in the year, as appears from Dr. Halley's table, and one in 30 at London, this difference does not shew, that the probabilities of life in both places are not the same above 20 years of age; for it may be fully accounted for, from the different probabilities under the age of 4. Because every year, at London, there dies one-fourth of the infants, under 4 years of age, more than at Breslau; which, without considering the other differences that may be between that age and 15, will more than account for the difference I have mentioned. At London the proportion of one in 30, comprehends all, as well strangers as natives; but at Breslau the proportion of one to 34 is only to be understood of those that are born there. Mr. Smart, in his table, has made the proportion among those that are born at London to be one to 24, and in his table corrected it is made as one to 19; but in the table that I have constructed from the Breslau and London together, it is one to 21,5; which is a medium between them.

If it be said, that from our bills the infant state, under the fifth year, certainly appears more unhealthy than at Breslau, and therefore probably it is so in advanced life: It may be answered, that possibly our gross air may not suit so well with infants, and yet may agree well enough with them after they become stronger; as we see some sorts of food are improper for the infant state, and yet do very well when nature comes to maturity. And it may likewise be alleged, that perhaps we are in a bad way of



managing of them, that many are destroyed, with sleepy and poisonous cordials, and others lost thro' the want of care and tenderness, or wickedness of our parish-nurses; for I know that there is not one in five survives their management.

And thus, from all these considerations, I think it may be allowed, until it is otherwise demonstrated, by bills formed in a different manner from what we have at present, that the probabilities of life are much the same at London as at Breslau, at the age of 20 or after 14. And if we take this for granted, we shall from thence be able to form a useful table, for those within our bills, by accommodating and joining the bills of both places together. And we may also nearly determine the number of infants born here, which hitherto has not been considered.

To find the number of births, by which I mean all those that are born alive, so as they might have been baptized, we must have the number of burials known, at least in the several periods, till the 20th year; *viz.* under 2, between 2 and 5, between 5 and 10, and between 10 and 20. And it is evident, if we suppose no accession of strangers, that the number of living in any one year will be equal to the difference between the births, and the sum of all the subsequent burials at each age till that year. The number of the living in any one year is easily known, if we suppose the probability of life to be the same as at Breslau; for then the number of dead there will be to the number of living, as the dead at London to the living. Thus in the 20th year the dead and living at Breslau are as 6 and 598, and the dead at London are 73, or more exactly 72, 88; therefore

the living must be 7263. The dead in the intermediate years at London may likewise be found, by means of Dr. Halley's table. For, by proportion, if the dead at Breslau from the age of 10 to 20 complete be 61, and in the 20th year 6, and the dead at London for the same period be 741; then will the dead in the 20th year be 73. And therefore if the living at London, in the 20th year of their age, be found to be 7263; this must be equal to the number of births, having subtracted from them all the dead in each of the preceding nineteen years. And consequently if we put  $x$  for the number of births, we shall have this simple equation,

$x - 8819 - 2006 - 805 - 741 + 73 = 7263$ ; and thence the number of births  $x = 19561$ . And the same number would have been produced from any intermediate age, between 12 and 20. So that if we could be certain of the number of the dead, there could be no doubt but, that 19561 would nearly at an average for ten years, be the whole of the births yearly. And this is greater than the number of baptisms known 14626, taken likewise at a medium, for the same ten years, from 1743 to 1753 inclusive, by the number 4935.

From which, by the way, we may see, as this difference between the births and baptisms must be occasioned by dissenters, that the number of such of all denominations, both protestant and popish, with the Jews, do not make above one-fourth of the whole of the people within the bills of mortality; and consequently that the protestant dissenters, exclusive of Quakers and Jews, are not above an eight part of the whole. And we may also observe, that as the difference between the births

19561, and burials 24867 is 5306, there must be a constant supply, yearly, of at least 5000 strangers, to keep up the people within the bills, to their present number: And the births are to the dead yearly, about 4 to 5.

If we had, in the same manner, computed the births from Mr. Smart's corrected table, they would have been found to be 17992, that is, 1569 fewer than we have made them. And if from thence we had calculated the number of people living to 20 years of age, and afterwards, by proportion, to 90, the whole number of people within the bills would have been about 521000; which is above 150000 fewer than any other reasonable calculation can make them; which I think clearly shews, that the hypothesis upon which that table is founded must be wrong, and that what I have laid down above is nearer to the truth.

Now, from the births found 19561, and the numbers of the dead in the different periods known by our bills, it will be easy to form a table of the decrements of life; because the dead in the intermediate years may be found by what has been said above. And accordingly I have computed the following, which is constructed from the London and Breslau bills together; which I think is a surer method of computing for us at London, than from either of them alone. The first part to the 21st year, is done from our bills, and the other part from the Breslau; but it is formed in such a manner, that it goes on as if from the bills of one place only. For, after the age of 20, it is continued by proportion, by making the dead at London in the decennial periods, to have the same ratio to one another as the dead at  
Breslau.

Breslau. It supposes 1000 persons born in one year, and shews the annual decrease of them by death till 87 years of age, which may be considered as the utmost period of life. The intermediate numbers, marked *d*, shew the dead in each year. The use of this table is well known to all who can compute the value of annuities for lives.

Age.	Perf.	Age.	Perf.	Age.	Perf.	Age.	Perf.
1	1000	14	395	27	345	40	278
	323 <i>d</i>		4 <i>d</i>		4 <i>d</i>		6 <i>d</i>
2	677	15	391	28	341	41	272
	127 <i>d</i>		4 <i>d</i>		5 <i>d</i>		6 <i>d</i>
3	550	16	387	29	336	42	266
	45 <i>d</i>		3 <i>d</i>		5 <i>d</i>		6 <i>d</i>
4	505	17	384	30	331	43	260
	32 <i>d</i>		4 <i>d</i>		5 <i>d</i>		6 <i>d</i>
5	473	18	380	31	326	44	254
	26 <i>d</i>		4 <i>d</i>		5 <i>d</i>		6 <i>d</i>
6	447	19	376	32	321	45	248
	13 <i>d</i>		3 <i>d</i>		5 <i>d</i>		6 <i>d</i>
7	434	20	373	33	316	46	242
	9 <i>d</i>		4 <i>d</i>		5 <i>d</i>		6 <i>d</i>
8	425	21	369	34	311	47	236
	7 <i>d</i>		4 <i>d</i>		5 <i>d</i>		6 <i>d</i>
9	419	22	365	35	306	48	230
	6 <i>d</i>		4 <i>d</i>		6 <i>d</i>		7 <i>d</i>
10	413	23	361	36	300	49	223
	6 <i>d</i>		4 <i>d</i>		6 <i>d</i>		7 <i>d</i>
11	407	24	357	37	294	50	216
	4 <i>d</i>		4 <i>d</i>		5 <i>d</i>		7 <i>d</i>
12	403	25	353	38	289	51	209
	4 <i>d</i>		4 <i>d</i>		6 <i>d</i>		7 <i>d</i>
13	399	26	349	39	283	52	202
	4 <i>d</i>		4 <i>d</i>		5 <i>d</i>		7 <i>d</i>

Age.	Perf.	Age.	Perf.	Age.	Perf.	Age.	Perf.
53	195 7 d	62	130 7 d	71	73 7 d	80	17 4 d
54	188 6 d	63	123 6 d	72	66 7 d	81	13 4 d
55	182 6 d	64	117 6 d	73	59 7 d	82	9 3 d
56	176 6 d	65	111 6 d	74	52 6 d	83	6 2 d
57	170 6 d	66	105 6 d	75	46 6 d	84	4 1 d
58	164 6 d	67	99 7 d	76	40 6 d	85	3 1 d
59	158 6 d	68	92 6 d	77	34 6 d	86	2 1 d
60	142 6 d	69	86 6 d	78	28 6 d	87	1
61	136 6 d	70	80 7 d	79	22 5 d		

And now, my Lord, I doubt I have made this Letter too long; but the importance of the subject will, I hope, excuse me. And if I have been mistaken in any particulars, I must confide in your usual goodness to forgive me; for you are sensible of the difficulties that occur. My endeavours, if I have not succeeded, will perhaps excite others, who have more leisure, and greater abilities, to make farther inquiries. I have some other things upon other subjects which I shall presume shortly to offer to your consideration. And in the mean time I am, with the greatest affection, My Lord,

Sion-College,  
April 24, 1755.

Your Lordship's most devoted,  
and faithful servant,

W<sup>m</sup>. Brakenridge.

XXIX. *Some Account of a Sheep, shewed alive to the Royal Society, in November 1754, having a monstrous Horn growing from his Throat; the stuffed Skin of which, with the Horn in situ, is now in the Museum of the Society. By James Parsons, M. D. and F. R. S.*

Read May 1,  
1755.

**T**HIS animal was bred in Devonshire, with the preternatural horn appearing at its birth in much the same proportion as at present. The novelty of the thing made the farmer spare the life of the lamb, and bring it up till it grew to the size of a well-grown sheep, pretty large of its kind, and about three or four years old. When it was brought before the Society, I remember the owner said, the horn weighed then twenty-six pounds; and the creature swung it about, and raised it up with amazing strength. When he was fed, he moved forwards, letting the horn drag between his fore-legs, whereby he was enabled to lay his nose to the ground; for the skin, by which it hung, is flexible, and though reduced to a neck, with respect to the circumference of the horn, yet it was hollow as well as flexible, leaving an open passage from the flesh of the neck to the cavity of the horn, and its contents. Sometimes the horn would come into such positions, as to twist the skin, which gave the sheep great uneasiness; but, from necessity, he knew how to relieve himself, and from custom

custom became ready at that, as well as bringing it between his legs to favour his feeding.

That he was a well-grown sheep, appears from the following dimensions :

He was four feet six inches in length, from the nose to the setting on of the tail ; which tail was but six inches long ; and the size and weight of the horn was so great in proportion to the animal, that nothing but his having been accustomed to move, and carry it about from his birth, can account for the great power he shewed in his manner of command-it in every attitude.

Nature is ever busy in supporting herself : when a preternatural weight was to be carried about, the muscles of the neck had acquired a more than natural strength and robustness to answer it ; for it was very remarkable, that this creature elevated his head upon any occasion, with as much seeming ease, as if no weight had been suspended to his neck ; although, joined to so great a weight as twenty-six pounds avoirdupoize, the enormous size of the horn must make it very unwieldy and cumbersome, being in length along the convex or anterior surface two feet seven inches ; and on the concave side two feet one inch ; its greatest circumference two feet two inches, middle circumference one foot six inches ; and near the apex one foot ; and its weight is now fifteen pounds, though emptied of its contents.

It is said, that the sheep in Devonshire have their natural horns of the shape of this preternatural one ; whereas they are curled in those of other counties. Now this animal had in the natural places no horns, but only two horny stumps, projecting no more than  
half

half a walnut-shell laid upon a flat surface, with the convex side upwards; for the horny particles, which, according to nature, ought to have had their secretion to produce horns in the usual places, were determined to this monstrous one perpetually, in the manner, which I have already attempted to explain, in the chapter of the analogy between the fluids of animals and vegetables, of my late treatise, intituled, *Philosophical observations upon the analogy between the propagation of animals and vegetables, &c.*

It is said by the person, who brought the stuffed skin of this sheep to the Society last Thursday, that upon opening him there was found, in the top of the horn next the throat, which is hollow half-way down, a skull of a contracted round form, with blood-vessels running upon it, and a bag filled with grumous blood, among which was a substance like a sheep's liver and lungs; and a perfect sound kidney, like that of a fresh loin of mutton. And this is attested by the names of three house-keepers of credit, who were present when the animal was opened, and who, if required, are ready to make oath of it.

If this be true, the case of this sheep is of the same nature with those of the cow, which was shewed in the museum, to the whole Society, in November 1748, having the parts of a twin-sister adhering to the spine, and prominent upon her back: and also of an ox, which was shewn in town about that time, having the head of a twin-calf hanging from between the sides of the under jaw, by a narrow skin.

As to the manner, in which such monstrous productions are formed, in order to avoid an unnecessary repetition, I refer to my account of two female



children, joined together by the bellies, in N<sup>o</sup> 489. page 527. of the *Philosophical Transactions*, where, in my remarks upon them, I have attempted explaining the phænomena of all these preternatural appearances in animal bodies, as well as in those of vegetables.

XXX. *A Dissertation upon the Cancer of the Eye-lids, Nose, great Angle of the Eye, and its neighbouring Parts, commonly called the Noli-me-tangere, deemed hitherto incurable by both Antients and Moderns; but now shewn to be as curable as other Distempers. Addressed to the Royal Society of London by Monsr. Daviel, consulting Surgeon in ordinary, and Oculist to the King; Master of Arts, and of Surgery at Marseilles; Royal Professor and Demonstrator of Anatomy of the same City; Member of the Academy of Sciences of Toulouse, Bologne, and that of Surgery of Paris; and translated from the French by James Parsons, M. D. and F. R. S.*

Paris, April 20, 1754.

Read May 8,  
1755.

OF all the diseases which seize the eye-lids, nose, angle of the eye, and its neighbouring parts, none appears so formidable

as the cancer, in the opinion even of the most able oculists, who have written upon this subject, such as Antoine Maitre Jean, St. Yves, and others. They have constantly declared it to be incurable in these parts, and have even forbid meddling with it: Antoine Maitre Jean says, "The operation is so doubtful, that it is rejected by the best practitioners, not only for cancers of the lids, but also for all those of the face, &c." And St. Yves is of the same sentiment, where he says, "That when the edges of the ulcer are accompanied with callosities, there is nothing to be done but by palliatives."

The patients would have reason to complain, if we were willing to have regard to what these authors have advanced, as well as others, upon the same subject, who were of the same opinion. The most able oculists have indeed met so many difficulties in this case, that they entered into an opinion, that they were impossible to be cured; and therefore never dared to undertake them. Some are content to treat them with palliatives, such as frog-spawn-water, and other such ingredients, as serve only to amuse the patient; whilst others, more bold, touch them with liquid caustics, or the lapis infernalis, from which they have had no better success than from the application of the water; because the best managed caustics only serve to irritate these kinds of tumors, as experience has too often shewed. But why therefore should these parts be incurable? Is it that they differ from the other parts of the human body? Certainly no. Why should these therefore be thought more desperate than the breasts, lips, and many other parts, which often yield to the knife,

when directed by a skilful hand : I will venture to say, it is indolence, little or no experience, and an ill-grounded fear, in oculists, both ancient and modern, that made them believe these kinds of diseases ought to be treated differently from all others. A bad prejudice ! which yet seduces a great part of our most able practitioners : but it must be averred, that they are deceived, if, in order to put it upon the footing of an ill-grounded fear, we must say, it has often hindered them from helping a number of patients, who have perished in a miserable manner, by not properly attempting their cure. I am willing to free myself from so hard a law ; I reprimand both antients and moderns, and the opportunities I have had of operating upon cancers of the lids and face, easily shew'd me, that they were very curable, and that the cure ought not to be given up to an uncertain issue.

The examinations I made in these kinds of tumors have informed me, that cancers of the lids, nose, and adjacent parts, have all their seat in the Periosteum, and Perichondrium ; and that we cannot hope for a thorough cure, without taking them intirely off : in a word, the vessels that go from the cancerous tumor are strongly connected with the Periosteum and Perichondrium, that they seem but one body, which becomes at length so greatly swelled, that the very bone is often affected.

When a wen or wart (which is often the beginning of a cancer), begins to appear, and they endeavour to pull them off, they become irritated, and spread to that degree, that the edges are reversed, and become callous and livid, accompanied with a  
 pain,

pain, and every other symptom which characterise the cancer. These kinds of wens, warts, and tubercles, which are situated in the great angle of the eye, upon the lids, or the nose, very often shoot out their roots upon the cartilages, that is, upon the very membranes which cover them, and the roots sink in sometimes to the substance of the cartilage itself, which they swell and tear in the end.

The more cancers are touched with caustics, the more they are irritated; therefore there is but one method, but it is a sure one, of curing them, and hindering their progress; which is, to take them off with a cutting instrument, destroying the Periosteum and Perichondrium, or even the lids, if the cancer has penetrated them in their substance, with their cartilages: which the following observations will prove.

Observation I. upon a cancerous upper-lid.

August the 11th, 1736, I was called to Madam de la Fague, an Ursuline Nun, at Bourdeaux, forty-five years old; to see a tumor upon the upper-lid of the right eye, which she had for twenty years: it begun by a small wen, and increased by degrees, so as very much to incommode the patient.

She applied to a surgeon, who began by applying some drops of a liquid caustic, which enraged the tumor still more; which he appeased again by anodyne medicines; and then the tumor remain'd a long time without any sensible increase; although the patient felt a continual sharp pain in it. But, as even the least disorders are impatiently borne, she was willing to be relieved, and consulted another surgeon, who took off the tumor with a cutting instru-

instrument, and who, seeing that the ulcer, which was the result of the operation, did not heal; but on the contrary made great progress in its erosion, and became callous, he touched it with lapis infernalis; and sometimes with a liquid caustic: which so much the more increased the evil, and made her resolve to suffer no more applications, because, all that had been tried, made her worse and worse. She was now a long time in this state, when I was called to consult with Messieurs Dotiasan, Caudole, and Senis, physicians of note in the same town, and with Monsieur Costade, surgeon major of the hospital; who, having examined the case, agreed with me, that there was no other method to be taken but the operation, not only to save the eye, but to prevent an incurable cancer, which threatened her life; whilst it had already made great progress under the eye-lid; and it was much to be feared that it would spread into the very eye, and even over the whole face. Then the operation would be fruitless, and the patient would suffer the loss of the use of that eye, and perhaps of her life. Wherefore I did not hesitate to propose the total extirpation of the lid: my proposal was approved of by all, as the only method of saving the eye; and the operation was as follows:

I passed a crooked needle, with a wax'd thread, under the lid, by which I suspended and drew up the lid and tumor, which I cut off with my crook'd scissars, as much as I could under the orbit, separating the whole to the division of the lids; a small hæmorrhage ensued, but was soon stopped with dry lint, and a dry compress and bandage.

She

She remained twenty-four hours without being dressed; was bled twice in the arm, after the operation: I then dressed her up with light doffils, arm'd with the linimentum Arcaei, and she had not the least accident from the day of the operation to the 25<sup>th</sup> of the same month, when she was perfectly cured, without any deformity in her eye: and although the lid was cut away very high, the eye remained very neat and well, performing its several functions properly when I left Bourdeaux; and the 13<sup>th</sup> of August 1742, having had an opportunity of taking a journey to that town, I saw the patient again, whom I found extremely well, seeing perfectly with that eye: but what I found very singular was, that the skin of the lid descended pretty low, to the cornea, which it almost covered; so that the whole globe was in a manner hid. We only observed, that this resembled a lid without hairs.

Observation II. upon another cancerous tumor in the great angle of the eye.

July 2, 1736. Margaret Combaucut, of Carcastone in Languedoc, sixty years old, had a cancerous tumor, for sixteen years, in the great angle of the right eye: it began by a little wart, which itched violently, and made her scratch it very often, which so irritated the tumor, that in a little time it became as large as a dried fig flatted, with its edges turn'd outward and callous. It reached from the commissure of the lower lid, an inch and half below it, even to the right ala of the nose, which proved extremely troublesome to the woman. I found, after a strict examination, that it adhered to the bone. She said she tried all the remedies that she imagined would

do her any good; but that, far from relieving her, they rather made her worse, and her disease became the more insupportable; and that she had taken a resolution to undergo any thing to be freed from a disorder which had afflicted her for sixteen years.

Having consulted Mr. Fabre, an able physician of that place, we were both of opinion, that she could not be cured without an operation, which I accordingly proceeded to as follows: I took off the tumor intirely to the periosteum, but did not lay the bone bare; for I thought it sufficient for a complete cure to take away all the callosities; but I was mistaken; for, instead of the prospect of a succeeding cure, I was unhappy enough to see the swelling increase, and the wound seem larger than before. I used in vain all the remedies commonly thought of in such cases; I scarified the edges of the ulcer, to bring it to suppuration; but it became thereby more hard and callous than before the operation, and much more painful. Upon which I resolved to cut away all that remained of the tumor, with the periosteum, which appeared very much swelled. This second operation had so much success, that the swelling, and every other bad symptom, disappeared almost suddenly; and in three days the wound looked red and very well, without any pain, and the cicatrix was perfectly form'd on the 15th day from the operation, without any sensible exfoliation of the bone, or the least deformity or staring of the eye. She has remained very well ever since; for I saw her the 10th of August 1741, at Carcastone, in perfect health; and the cicatrix of the part very even.

I must observe here, that I laid the intire bone bare, wherever the tumor touched, even down to the ala of the nose of that side.

Observation the 3d, is upon a cancerous tumor of the same nature, and in the same situation, and the treatment just the same: it was as big as a filbert, and the officer was afflicted with it twenty years.

It differs from the former only in this, that the year before the officer came to Marseilles, to put himself under Monsieur Daviel's cure, the tumor broke, and discharged a very fetid acrimonious matter, which running into the eye, brought on a troublesome ophthalmia, and the edges were livid, and had a very terrible aspect. As to his operation, it consisted, as before, of a total extirpation of the cancer, periosteum and all, to the bare bone. He dressed the bone with dry lint only, and his digestive was a mixture of the linimentum Arcæi, with the unguentum styracis: and in about nineteen days he was so perfectly cured, that when he returned to his friends, several of them asked him, upon which eye the operation had been made?

Observation 4, differs in nothing from the former.

Observation 5, upon a cancerous tumor upon the nose, which reached from the root of the nose down to the middle of the cartilage. He treated it in the same manner, taking off the whole with the periosteum; and, as it was partly upon the cartilage, he also cut away the perichondrium, laying that, as well as the bone, bare: and the cure was completed, without leaving any deformity behind, in eighteen days.



Observation 6, of a cancerous tumor upon the great angle of the right eye of a woman at Marfeilles, of seventy years old. This he treated exactly in the same manner, and she was cured in twenty days.

The 7th Observation mentions another cancerous tumor upon the nose, and its cartilage, of a gentleman, which is circumstantially the same with the former: it was cured in five days. After this case he makes this conclusion: that from all that has been already said, it is plain, that the seat of the cancers of the eye-lids, nose, and other neighbouring parts, is absolutely in the periosteum and perichondrium, as well as the fat; and that there can be no hopes of a cure without taking off these membranes, the fat, and even any parts of the very cartilages that may be contaminated: but that in this manner they are as curable as cancers upon other parts of the body, notwithstanding what all oculists have said to the contrary.

The 8th Observation is upon a cancer upon the lower eye-lid of a woman, cured in the same manner.

The 9th Observation treats of a cancer, as big as a large filbert, in the angle, and upon the lower lid of the eye, of a gentleman; which began by a small tubercle in the angle, and was pulled off, and grew again several times. Monsieur Daviel was consulted, in the presence of another surgeon, Monsieur Maillot, and declared for taking the tumor and eye-lid intirely off; making this prognostic, that if any part was left behind, the eye would be deformed and staring; but the other surgeon thought, that half the lid with

the tumor would be sufficient for the cure: Monf. Daviel therefore only cut away half the lid with the tumor; with which he alfo took off a large quantity of hard white fat, and dressed up the part as usual; but in the progress the lid was turned outward, and then they resolved upon the total extirpation of the lid; which, being obliged to depart from thence, he left to Monf. Maillot, who performed it with such success, that his cure was complete in fifteen days, without the least deformity whatever.

The 10th Observation is a case of the same nature with the former, with this difference, that when he had taken off the tumor and under lid in the same manner as usual; the patient continued growing well till the ninth day from the operation; when Monf. Daviel perceived a small fungus in the middle of the tumor, which he touched with the lapis infernalis, which produced very ill effects: the eye grew painful, the conjunctive swelled very much, the wound, which was half healed up, opened afresh, and grew ragged. This made him set about cutting away all the bad flesh he could perceive, with the inequalities of the conjunctive, which was much swelled: he scarified the cornea, and the inner surface of the upper lid, which was also greatly tumefied, and even opened it on the upper surface. Thus, after having emptied the vessels well, he fomented the whole with a decoction of marshmallows, mullein, violet-leaves, camomile-flowers, melilot, leaves and flowers of rosemary, thyme, lavender, rue, and marjoram, of each half a handful, in a sufficient quantity of water; to a quart of which he put a bit of camphire as big as a nut. The frequent applica-

tion of this that day produced so good an effect, that all her pain ceased : he also bled her in the arm and foot, ordering emollient clysters. She was purged some days after, with manna and cassia, which did very well ; and she was perfectly cured, without the least deformity, and could see better than before the operation.

XXXI. *An Account of four Roman Inscriptions, cut upon three large Stones, found in a ploughed field near Wroxeter in Shropshire, in the year 1752: With some Observations upon them, by John Ward, L.L.D. Rhet. Prof. Gresh. and V. P. R. S.*

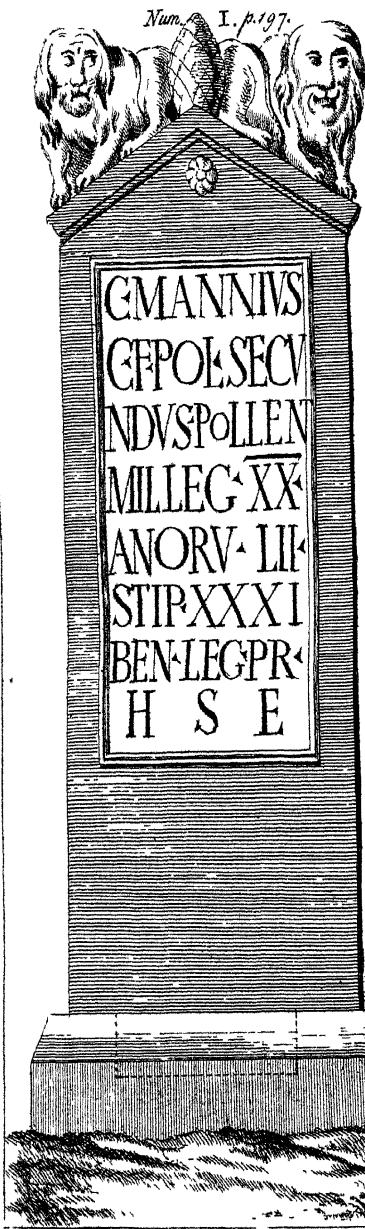
Read May 15,  
1755.

**B**EFORE I attempted to offer my thoughts upon these inscriptions, I judged it necessary to get the best information in my power, with regard to the place and manner, in which the stones, that contained them, were first discovered, together with some other circumstances, which attended them at that time. For this purpose I applied myself to the Reverend Mr. William Adams, Minister of St. Chad in Shrewsbury, by whose means the draughts of these inscriptions were communicated to this Society (1). And that gentleman was so obliging, as to procure for me a very particular account concerning them, in a letter from the Reverend Mr. Robert Cartwright, Vicar of Wroxeter, the substance of which is as follows. The stones

(1) See TAB. V.



*Num. I. p. 197.*



*Num. II. p. 198.*



1 2 3 Feet





stones were found in a feild near a mile from Wroxeter, formerly a Roman station called *Uriconium* (1), in the months of September and October 1752. The first of them was discovered by Mr. John Dias, the owner of the feild, whose plough struck against it, as he was at work. It lay with the face downward, about two hundred yards from the foundation of the old walls, on the north east side, towards Watlingstreet road. The inscription upon this stone was first taken notice of by Mr. Cartwright himself, and the report of it soon after engaged some gentlemen from Shrewsbury, and particularly George Edwards esquire, both to go and view it, and make a further search; when by spitting the ground the other two were discovered, not far from the first, in the like situation. The first and last lay separate from their bases, which being taken up, several broken peices of urns, and dust of a greyish colour, were found with them, which seemed to have the appearance of ashes. They are now all removed to an old chancel in Wroxeter church, in order to be erected there against the wall.

This account appears so full and distinct, that I shall now go on to give a short description of the stones, with the reading of their several inscriptions, in the order they are placed in the *Table*; and then add some further observations, as well with regard to the stones themselves, as the inscriptions upon them.

*Number 1.* is by the scale six feet eight inches high, and about two feet three inches wide above the

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(1) In the year 1701, a Roman sudatory was discovered at this place, a draught whereof, with some account of it, was published in the *Philosophical Transactions*, Num. 306. which seems to have escaped the observation of Horfley, *Brit. Rem.* p. 419.



base. It has a pediment top, with a pine apple rising from the middle of the cornice, on each side of which is a lion, and in the area of the pediment a kind of rose. The inscription, which is cut in the plane of the stone, may be thus read: *Caius Man-  
nius, Caii filius, Pollia tribu, Secundus Pollentinus,  
miles legionis vicefinæ, annorum LII, stipendiorum  
XXXI, beneficiarius legati principalis, hic situs est.*

Number II. contains two inscriptions, and is in height two feet seven inches and about a third, by two feet four inches and two thirds in breadth. It is not flat, as the former, but gently convex cross-ways, the lower part being divided into three panels; on the two first of which are the inscriptions, but the other seems never to have had any upon it. The upper part is ornamented with a pediment, in the area of which are the remains of a face with curled locks, and two snakes under it; and on the cornice two figures like dolphins. The first inscription may be read thus: *Diis Manibus. Placida annorum LV,  
curam agente conjuge annorum xxx.* And the other in this manner: *Diis Manibus. Deuccus annorum xv,  
curam agente patre.*

Number III. is six feet eleven inches high, and about two feet broad above the base. It has also a pediment at the top, the area of which is filled with a large flower. The inscription, it exhibits, may be read in the following manner: *Marcus Petronius,  
Lucii filius, Menenia tribu, vixit annos xxxviii,  
miles legionis XIII geminæ, militavit annos xviii,  
signifer fuit, hic sepultus est.*

As these readings of the several inscriptions appear in general very plain and obvious; I shall now proceed to make a few observations, partly on the ornaments

naments of the stones, and partly on some particular circumstances relating to the inscriptions themselves. And

1. As to the ornaments on *Number I*, we meet with the pine apple upon several urns, published by Montfaucon (1) and others; and it seems to have been cut on some funeral monuments in Horsley, tho now it is generally pretty much defaced. The rose likewise is very common on such occasions. Horsley has also given us a sepulchral monument, with a lion placed on each side of the *fastigium*, or pediment (2).

With regard to the inscription, the name *Secundus* stands there for the *cognomen*; which at first was a *praenomen*, denoting the second son, as *Primus* did the first, and *Tertius* the third. But afterwards it was made a *cognomen*; and what was before the *cognomen* became hereditary, and served to distinguish different branches of the same family; as I have formerly shewn elsewhere (3). The next word, *Pollentinus*, comes from *Pollentia*, the place of his birth. But as there were antiently three Roman cities of that name, it is uncertain, which might be intended. He is called *miles legionis vicefimae*, which was one of those legions, as Horsley observes (4), that came

(1) *Tom. v. plate 28, 33, 38, and 62.*

(2) *Cumberl. xxxix.* Mr. Cartwright in his letter observes, that the tongues of the lions, cut upon this stone, hang out of their mouths below their under jaws; which has been omitted in the draught here given of them.

(3) *Philos. Transf. N. 476, pag. 357.*

(4) *Page 83.*

over into Britain in the reign of *Claudius*. But in all the British inscriptions hitherto published, where the reading is certain, the titles *valens victrix*, denoted by the initial letters V. V. are added to the name of this legion (1). And how they came to be omitted here, unless thro neglect of the workman, may be difficult to apprehend. For to imagine this inscription was made, before the legion received those titles, would be mere conjecture, without any authority to support it. The last line but one has been read *beneficiarius legionis praefecti*. And we find indeed the title *praefectus legionis* in some of Gruter's inscriptions; but this seems not to have been introduced till pretty late, when each legion had its *legatus*, who is sometimes also called *praefectus* (2). But if that was to be taken here for the genuine reading, it would bring this inscription too low for the form of the letters, which seems very well to agree with the times of the higher empire. I have therefore given another reading of it above, which was proposed by a learned friend, namely, *beneficiarius legati principalis*; since in one of Gruter's (3) inscriptions we meet with *principalis beneficiarius tribuni*, written in words at length.

2. As to *Number II*, the human face, with two snakes under it, might probably be designed to represent *Hecate*, a name given to *Diana*, as a deity of the infernal regions. A figure somewhat like this,

(1) See *Brit. Rom. Northum.* lxxviii. *Chefb.* i. and *Somerset.* iii. where the reading is uncertain.

(2) See *Mont. Tom.* iv. p. 13. and *Supplem. Tom.* v. p. 92.

(3) *Pag.* DLI. 3.

but with the snakes above the head, was placed over the gate of the city Ephesus, a draught of which is published by Mr. Chishull (1). Indeed the *trigla*, or mullet, is said by mythologists to have been consecrated to *Hecate* (2); but there is no accounting for all the different fancies of sculptors. In Horsley we find two dolphins placed in a contrary attitude, with their heads upwards, one on each side of a human figure, and a pine apple over it (3). And as garlands were a common ornament of such monuments, one hangs across each panel of this stone, above the inscriptions.

In the first of these inscriptions the form of the expression, *conjux triginta annorum*, is what I do not remember to have met with elsewhere; and the point on each side the letter I. after the abbreviated word CON. must, I presume, be ascribed to an error of the workman. And likewise in the second inscription, the division of the word DEVC·CVS, by a point in the middle; the omitting the letter A. in the abbreviated word AG. for *agente*, and afterwards interlining it; and the imperfect letter R. for P. in the word *patre*, may best be accounted for in that way. The third panel might be left vacant, in order to insert some other inscription afterwards; and the lower part of the stone is now wanting below the inscriptions.

III. In *Number III*, the ornamental part of the stone has nothing in it, that requires observation.

(1) *Antiq. Asiat. Par. alter. pag. 1.*

(2) See *Voss. De idololatr. L. ii. c. 29. p. 167.*

(3) *Durham. iv.*

And as to the inscription, the abbreviated word VIC. may doubtless stand for *vicist*, the letter x. being resolved into cs. And so we find the word *vicist* for *vixit*, written at length in Horsley (1). But the name of the legion here mentioned, which is called *quartadecima gemina*, may seem to be attended with no small difficulty. The stone happens to be cracked quite thro, just across the numeral figures, which express the number of the legion; but in such a manner, as no ways to deface or obscure them. Now we learn from Tacitus, that four Roman legions were sent into Britain in the reign of the emperor Claudius. Two of these were the twentieth and the fourteenth, both which are mentioned in these inscriptions. And the former of them remained here the greatest part of the time at least, that the Romans continued in possession of the island. But the latter was ordered hence by Nero, then sent back by Vitellius, and being recalled again at the beginning of Vespasian's reign, does not appear to have returned any more. "And that happening (as Horsley observes) before the opportunity or humour of erecting inscriptions obtained; it is not to be wondered at, that we do not find this fourteenth legion mentioned in any inscription (2)." However, this Wroxeter stone gives us both the name of that legion, and an additional title of *gemina* subjoined to it; the origin of which title seems to have been owing to Pompey, who had, as Caesar says, among

(1) *Cumberl.* lxx.

(2) *Page* 80.

his other legions of Roman citizens, *unam ex Sicilia veteranam, quam factam ex duabus gemellam appellavit* (1). That there was a legion, which bore the title of *quartadecima gemina*, is evident from several of Gruter's inscriptions; but they all relate to foreign countries, and not one of them to Britain. It is mentioned also by Dion, as being in his time, that is, under the reign of Severus, stationed in *Pannonia* (2). But it appears likewise from Caesar, that he himself had the *legio quartadecima* with him in Spain, not long after the commencement of the civil war (3), before the title *gemella* was introduced by Pompey. And therefore had the title *gemina* been given to the fourteenth legion, either before or while it was in Britain; it can scarce be supposed, that Tacitus, who so often mentions and extols that legion for its great services done here, should intirely have omitted it, especially as he gives that title to the *legio tertiadecima* (4). Sir Henry Savile indeed reckons the *legio quartadecima* among those of Galba, and ascribes to it the title of *gemina* (5). But he produces no other authority for it, than the passage before mentioned from Dion, who there speaks of this legion as having that title in his own time, without any intimation, when it first received it. Besides, that it did not receive the title of *gemina* during its residence in Britain, seems further evident from hence, that we find

(1) *B. C. Lib. iii. cap. 3.*

(2) *Lib. lv. pag. 564. edit. Leunclav.*

(3) *B. C. Lib. i. cap. 44.*

(4) *Hist. Lib. iii. cap. 7.*

(5) *Translation of Tacitus, sub fin. p. 218.*

no other legion here at that time, with which it could have been incorporated. For the other three, which continued longer among us, namely, the *legio secunda Augusta*, *legio nona*, and *legio vicesima valens victrix* remained distinct afterwards, and are all particularly mentioned by those different names.

I shall only observe further, that as there are two legionary foldiers named in this and the first inscription, mention is there made of the tribe, to which each of them belonged, as Roman citizens; whereas in Horsley we have the monuments of three foldiers of the *legio secunda Augusta*, without any mention of their tribes (1). There is indeed at Bath an inscription of another foldier, said to have belonged to the *legio secunda adjutrix*, whose tribe is there expressed. But as that legion does not appear to have ever been in Britain, he may be supposed to have come hither for his health, for which reason it might be thought proper to mention that circumstance. And perhaps the most probable way of accounting for the same thing, in relation to the persons nominated in the first and last of these inscriptions now under consideration, may be to suppose, that the character here given of the former, as *beneficiarius legati principalis*, might occasion that regard being paid to his memory in this funeral monument. And the latter might come over on some particular occasion, not here mentioned, while the *legio quartadecima gemina*, to which he belonged, was stationed elsewhere. But several gentlemen in that neighbourhood designing

(1) *Scotl.* xxxiii. *Astonum*. I. *Midd.* I.

shortly, as I am informed, to renew their search in and near the place, where these inscriptions were found; it is to be hoped, that some further light may by that means be gained, for the clearing up these difficulties.

XXXII. *Some Observations upon an American Wasps-Nest, shewn to the Royal Society : By Mr. Israel Mauduit, F.R.S.*

Read May 15, 1755. **M**R. de Reaumur distinguishes wasps into three classes, from the different situations, in which they place their nests; some making choice of unfrequented parts of houses, some of little cavities in the earth, and others of the branches of trees for that purpose. The first of these is the largest sort, or hornet; the second is the common sort here in England; and the last is more frequent in America.

The nest, which I now do myself the honour to communicate to this Society, was sent me from Maryland; where they are found on the lower kinds of trees, in the thickest parts of their woods. This is built upon a dogwood-tree, or the *Cornus mas Virginiana*; and hangs quite detached from the rest of the tree by an extreme branch, of little more than an inch circumference: which, with its smaller divisions running through the substance of the nest, answer the purpose of pillars, to unite and support the several floors of the building.



The figure is a conoid, or an acuminated oval: its longer diameter is twenty inches, the shorter near the base is twelve.

It is perforated on two opposite sides, for the inhabitants to enter and go out at: the upper door was originally less, but is enlarged, to give a more easy view of the structure of the cells within.

The shell is composed of paper; the sheets of which at its upper end are larger and more distinct. They are of an ash-colour, of different shades, and streak'd or marbled: and, being lightly laid upon each other, form a wall of from one and an half to four inches thickness in the several parts of it.

The lax hollow manner, in which they are joined to each other, renders them a more effectual security from rain; as they attract water in common with all other substances, made of the same materials; and would have been more easily soaked thro', if they had been closer compressed together. For the same reason the apex of the cone is of the greatest thickness; and the base is of a stiffer and more cellulose texture.

Our paper is formed of broken vegetable fibres, placed together without any order, just as the water leaves them upon the sieve. *This*, to the naked eye, yields the appearance of a more regular arrangement of its parts; being wholly composed of parallel striæ, placed at about a line's distance from each other; with fibres going off from them, in a manner imperfectly resembling the ribs and plumes of a feather. The microscope shews very little of this regular arrangement; except that the ribs consist of a thicker and closer texture; being the joinings of the  
several

several breadths, in which the animal works it. In all other respects it appears to be a plexus of fibres, very rudely platted together, and no way superior to the meanest sort of whited-brown paper, with which I have compared it.

In respect of strength it is much inferior to it; having nothing of that toughness, requisite to every other use of paper, except that one, for which this is intended.

Upon this single account the more rigid nature of its woody fibres is an advantage to it; as it is less bibulous than the common blotting paper, retains its stiffness better when it is wetted, and does not collapse so soon.

The colour is the same with that of oak-wood, and the common sorts of timber, after it has been for three or four years exposed in the air; and appears in the microscope to be intirely owing to extraneous matter adhering to it: for the fibres themselves are of a clear white, little differing from those of white paper, with this fordes scattered over them.

It burns like paper, but with a weaker and quicker flame, as if a great part of its oil had been before exhausted: and its smell in burning discovers nothing of the pungency of volatile salts, but proves it to be a purely vegetable substance.

Each of these observations confirms Mr. de Reaumur's account of its formation; it being composed of the fibres of wood, that is in its first stage of decay; when, by having been long exposed in the air to the action of the sun and rain, its external parts begin to separate, and give these animals an opportunity to tear off certain smaller filaments, which

are

are then loofened; and which they collect together into a little ball; and, having moistened it to a kind of paste, spread it out with their talons and fore-feet, into its present form.

Hence the marbling of this paper is the necessary result of the method of its construction: for as each sheet consists of a number of fasciæ or breadths, equal to the reach of the animal, that spreads them; each of these fasciæ will be of a different shade, according to the several colours of the little bundles of fibres, collected by so many labourers from different materials.

Upon the whole, the substance before us is a true paper; but, by the exact œconomy of nature, wrought to that degree of perfection only, which was necessary to serve the single purpose it was intended for. Being examined by the microscope, it appears to be of a coarser grain, a shorter staple, and of a much looser texture; and is a rare, though not a singular instance, of a natural production falling far short of the artificial one of the same kind.

The inside structure of these nests is so well described by Mr. de Reaumur, that we cannot hope to discover any thing new in the opening it.

**XXXIII.** *An Extract of a Letter written by the Magistrates of the City of Mascali, in Sicily, and sent from their public Office to Naples, concerning a late Eruption of Mount Ætna. Translated from the Italian.*

Mascali, March 12, 1755.

Read May 29, 1755. **O**N Sunday the ninth of this March, about noon, Mount Ætna began to cast from its mouth a great quantity of flame and smoke, with a most horrible noise. At four of the clock on the same day the air became totally dark, and covered with black clouds; and at six a shower of stones, each of which weighed about three ounces, began to fall, not only all over the city of Mascali, and its territory, but all over the neighbourhood. This shower continued till a quarter after seven; so that by the darkness of the air, the fall of stones, and the horrible eruptions of the mountain, the day of judgment seemed to some to be at hand. After the stones had ceased falling, there succeeded a shower of black sand, which continued all the remainder of the night. The next morning, which was Monday, at eight of the clock there sprung from the bottom of the mountain, as it were, a river of water, which, in the space of half a quarter of an hour, not only overflowed to a considerable distance the rugged land, that is near the foot of the hill, but, upon the waters suddenly going off, levelled all the roughness and inequalities of the surface, and made

the whole a large plain of sand. A country fellow, who was present at so strange a sight, had the curiosity to touch this water, and thereby scalded the end of his fingers. The stones and sand, which remain where-ever the inundation of the water reached, differ in nothing from the stones and the sand of the sea, and have even the same saltness. This account, however fabulous it appears, is most exactly true. After the water had done flowing, there sprung from the same opening a small stream of fire, which lasted for twenty-four hours. On Tuesday, about a mile below this opening, there arose another stream of fire, which being in breadth about 400 feet, like a river, began to overflow the adjoining fields, and actually continues with the same course, having extended itself about two miles, and seeming to threaten the neighbourhood. We remain therefore in the greatest fear and terror, and in continual prayers.

*XXXIV. Some Account of the Charr-Fish, as found in North-Wales. In a Letter from the Rev. Mr. Farrington, of Dinas, near Caernarvon, to Mr. Thomas Collinson, of London. Communicated by Mr. Peter Collinson, F. R. S.*

Read May 29, 1755. **T**HIS species with us is called tor-goch, a compound of tor, the lower part of the belly, and goch, red; in English red-belly. This redness in the female, paler or deeper, according

according to the season, resembles that of the fins of a roach, a fish very common in many rivers of England, though we have none of them in this country. The male is not adorned with that beautiful hue, yet he is finely shaded, and marbled upon the back and sides with black streaks, upon a kind of pellucid light sky-coloured ground. The make is that of a trout, but much more elegant and delicate; insomuch that the vulgar hereabouts affirm, that a charr is nothing else but a trout in high season. Certainly there is a very great likeness, though in one respect the charr seems nearly allied to the eel and the tench, in being very slimy; and the cure and potting of charrs well depends very much upon cleansing and draining them of this mucilaginous quality. Whether you boil, stew, or fry them, they taste like a trout, but much more simple and insipid. They appear with us but at one season of the year, about the winter-solstice; their stay is of a short continuance, as if an act of necessity, and they were in haste to be gone to some more remote and private habitations. Three lakes or large pools, at the foot of Snowden, afford being and subsistence to this remarkable finny race: two of them (in our Gwyddhelian Language) we call Llynian I lanberris; *i. e.* the pools or lakes of Llanberris, or the parish of Llanberris. The upper pool is called Llyn-Ucha, and the lower one Llyn-Iffa. There is a communication between one and the other. About a fortnight in December the charrs make their appearance in both; never wandering far from the verge of these lakes, or the mouths of the rivers issuing from them; but traverse from one end to the other, and from shore to shore indifferently, or perchance

as the wind fits, in great bodies; so that it is a common thing to take in one net twenty or thirty dozen at a night in this place; and not above ten or a dozen fish in all at any other. Thus in winter frosts and rigours, they sport and play near the margins of the flood, and probably deposit their spawn, and continue their kind; but in the summer-heats they keep to the deep and center of water, abounding in mud and large stones, as the shoaler parts do with gravel: Providence with-holding from mankind this delicious morsel, when it is least fit to eat; for after Christmas they are seen no more till the following year. But the shortness of their stay in the two above-mentioned waters is made some amends for by a succeeding, though as short a season, in a pool in my parish, to which we give the name of Quellyn, from an ancient family so called, situated hard-by; for the charr appears here immediately after Christmas; and some, though very few indeed, are taken in the trout-net, even at midsummer, or rather at the two trout-seasons in summer. 'Tis remarked, that the fish have a larger growth one year than another: and lastly, I may add, that the whole number of charrs annually taken in the two pools of Llanberris does not amount to an hundred dozen.

XXXV. *A Method proposed to restore the Hearing, when injured from an Obstruction of the Tuba Eustachiana. By Mr. Jonathan Wathen, Surgeon, in Devonshire-Square.*

Read May 29, 1755. **W**Hatever obstructs that passage leading from the ear into the nose, called tuba Eustachiana, so as to hinder the ingress of the air through it into the cavity of the tympanum, is, I believe, universally esteemed destructive to the sense of hearing. Hippocrates observed, that in a quinsy of the fauces, the patient became deaf, by its compressing and closing up this tube (a). Many practical writers assert the same to have happened from adjacent ulcers, &c. (b); and I have known a swelled tonsil occasion deafness.

This canal opens into the lateral and anterior part of the cavity of the tympanum; is so shaped that it first decreases, as it descends towards the posterior part of the nose, becoming very narrow; then suddenly diverging, is much enlarged, opening into the

(a) Coac. 11. n. 35.

(b) Haller in Boerhav. de auditu, p. 380, and 416. Tulpius l. n. 35. a tumore palati. Valsalva, cap. v. pag. 90. a polypo. & ulcere (*viz.* a certain yeoman had an ulcer above the uvula, on the left side, which communicated with, and corroded part of, the orifice of the left tuba eustachiana; which, when he stopp'd with a tent dipped in medicine, he immediately lost his hearing in that ear, but recovered it as soon as the tent was taken out).



posterior part of the nose by an elliptic orifice, a little prominent, turning inwards and forward, placed laterally, and just above the velum pendulum palati. This canal then is composed of two distinct cones, the extremities of which unite together, but their bases diverge differently: it is likewise lined with a porous membrane, full of criptæ and mucous cells, continued from and like to the membrane of the nares (c).

When therefore we consider the structure of the eustachian tube, and its free communication with the atmosphere, we may reasonably suppose it subject to inflammation of its membrane, and concretion of its mucus, from cold, &c. like the external meatus; and although its mucus is of a very different nature, it is nevertheless liable to inspissate by heat, when its thinner parts are exhaled\*.

And from the form of this passage we may easily conceive, that an obstruction, pretty far advanced, is not to be removed without difficulty, and that in proportion, as it is more or less complete, the hearing will be more or less injured. Why then may not this be suspected, as sometimes the cause of deafness? Perhaps it is not unfrequently so: *e. g.* When

(c) Haller in Boerh. de Auditu, pag. 378. Not. e Physiologia. Haller. de Auditu, §. 485. Valsalva, cap. 2. pag. 32. idem Fig. XIV.

\* Morgagni and others tell us, that they constantly find the cavity of the tympanum in infants always much clogged with mucus; and Mr. Douglas has often observed the same in adults, and is of opinion that it is concomitant with an obstructed tube in general, and that the injection is equally as effectual as if the tube only was obstructed.

a patient is somewhat deaf from cold, and the outer ear has been examined, and found clear of harden'd wax, &c. it is nevertheless not uncommon to find himself suddenly relieved by a great noise in his ear (*d*). This is probably owing to the breaking away of the congealed mucus, and the instantaneous rushing of the air into the tympanum; so that when this disorder is but slight and recent, nature seems frequently to relieve herself; but when more confirmed, her efforts are ineffectual for its removal.

These considerations inclined me strongly to think, the hearing might suffer from that cause, and I was much confirmed herein by the following very remarkable case.

Richard Evans, aged thirty-five, was exceeding deaf in both his ears, and no visible disorder in the external meatus. It arose from cold, and had subsisted several years, during which time no art or means whatsoever, could procure him the least relief. In August last he died of the small-pox, at the hospital in Cold-bath fields. I took that opportunity to examine the eustachian tube of each ear, and found them both stuffed quite full of congealed mucus, which was observed by two gentlemen of the profession present. This was the only visible cause of his deafness, the other parts appearing in their natural state.

As all these concurring circumstances strengthened me in my opinion, they likewise incited me to make trial of an operation that was some time ago proposed.

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(*d*) Haller in Boerhaav. de Auditu, pag. 381. not. g.

to the Academy of Sciences, by Monsieur Guyot ; but the author having never practised it, he wanted the recommendation of facts to support and enforce it ; it was therefore rejected by them as impracticable (*d*).

I first introduced my probe, a little bent at the end, through the nose, into the tubes of several dead subjects ; and, having thereby acquired a facility, I did the same on a person that was very deaf, and on whom all other means had proved ineffectual : no sooner had I withdrawn the probe, than he said, he could hear much better. This success excited my further endeavours, so that I had pipes of different sizes adapted to a syringe, and have since injected the meatus internus in the following manner, with success.

The pipe is made of silver, about the size and length of a common probe, and a little bent at the end : this being fixed to an ivory syringe, full of liquor (*viz.* a little mel rosarum in warm water), must be introduced between the ala and septum of the nose, with its convexity towards the upper part

(*d*) Hist. de l'Acad. 1724. pag. 53. Besides, Monsieur Guyot proposed doing it by the mouth, which is quite impossible, as evidently appears to any one that will give himself the trouble to examine into it. Convinced of this, Monsieur Petit (who has lately published a new edition of Palphin's anatomy) proposed, and that learned and skilful anatomist Mr. John Douglas first demonstrated the possibility of, passing the probe, &c. through the nose into the eustachian tube : and this he has constantly shewn to those who have attended his public lectures ; and to him I freely acknowledge myself indebted for the hint, by which I was incited to make trial on the living, of an operation of so much importance to mankind.

of

of the aperture of the nares; and thus continued backwards, and a little downwards, till it comes near the elliptic orifice; then its convexity is turned toward the septum, by which the inflected extremity enters the tuba eustachiana with ease: the liquor is then impelled through it into the tube, by which the *fordes*, if any, being diluted, is washed out, and regurgitates through the nose, or mouth, or both, with the injection; and, if the quantity be large, may be seen.

November 3, 1754. M—S— about forty years of age, being troubled with a very considerable deafness, so that she could not hear any thing said at a common distance, except the voice was very loud and shrill; people were therefore obliged to speak into her ears. This rendered her incapable of service; so that her mistress resolved to dismiss her: it was of two years continuance, but growing much worse of late, and originally caused by cold, I syringed her outward ears first of all, without the least benefit; but as soon as the internal meatus was injected, she instantly affirmed, that she heard much better; and by repeating it for two or three days, she heard, and continues to hear, almost as well as any body, and remains in her place.

November 17, 1754. S—L— aged fifty, applied to me for relief of a deafness in both ears, that had subsisted for a year and an half, and was the effect of a cold: he could not hear what was said, without a most violent exertion of the voice, and applying one's mouth close to his ear; nor could I make him hear at all; so that I was obliged to converse with him by means of a person that had a more loud and shrill voice. Having syringed his outward

ears without any success, the next day I injected the tube on one side, and washed away a very considerable quantity of congealed mucus, in little clots of a blackish colour and putrid smell, regurgitating with the liquor through his mouth, he immediately heard what was said by some persons talking in another part of the room. The morning following I did the other ear, and with the same success; and by repeating the operation for two or three times in as many successive days, he can now hear a common conversation; and, if near, distinguish what is said, though the voice be very soft and low, but cannot hear sounds at a great distance.

November 18, 1754, L— threescore years of age, having been exceeding deaf for thirty years, desired to have this operation performed on one of his ears. I first injected the external ear of the right side, and extracted a large plug of inspissated wax; but this did not relieve him in the least. The next day I syringed the tuba eustachiana of the same side; he could then distinctly hear the ticking of his watch, applied close to his ear, which he could not do before nor since: his deafness returned again in the evening: I repeated the operation two mornings more, whose effects were exactly the same as the first.

November 20, 1754, E— H— had been so exceeding deaf (from a cold) for six years, that she was incapable of any kind of employ whatever. I tried this operation, and continued its use, every other day for a fortnight. The benefit that she received, though not equal to the second case, is nevertheless so great, that she can now wait at table, hear what is said pretty well, though not spoke directly  
to

to her, and is become very useful in the family where she lives. This is the more extraordinary, as her external ears have a continual spasmodical motion, which indicates a disordered state of the nerves of her ears.

November 30, 1754, A— aged twenty-seven, deaf in both ears, from cold, and of two years standing, one much worse than the other, I began with the deafest, and extracted much wax, &c. from the external meatus, without the least benefit; but on syringing the tube of that ear, she received so much relief that she can hear considerably better with it than the other. I then injected the other ear, on which it produced no alteration at all, though repeated several times.

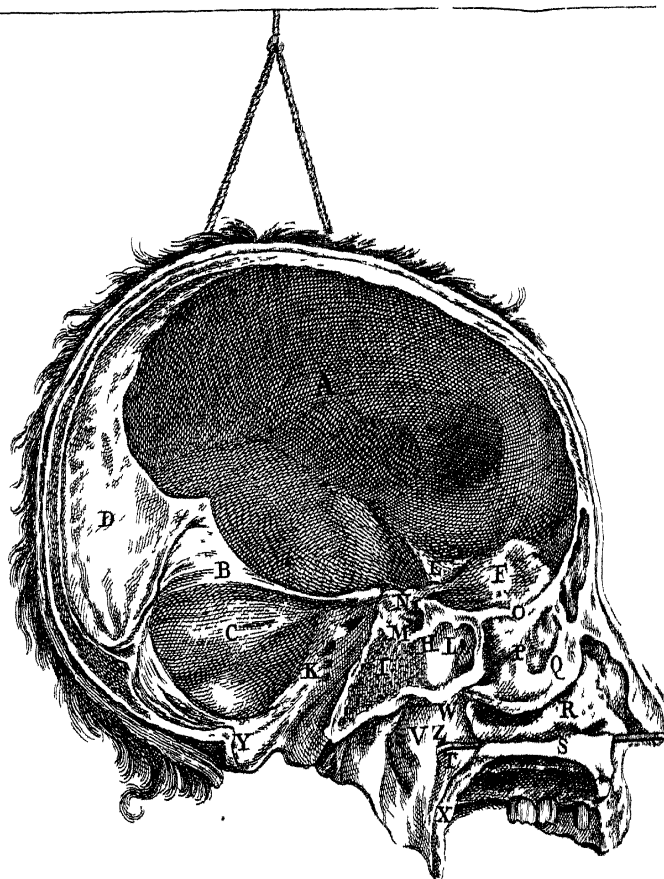
February 1, 1755, A— A— deaf to the greatest degree imaginable, could understand only one particular person, whose voice, or rather physiognomy, he had long been used to. He had been thus for eighteen years, and was suddenly seized, or as it were struck, with this disorder, together with an affection of his eyes, which presented a variety of colours continually floating before them, to the great detriment of his sight; and this, together with his deafness, has continued, with very little alteration, till the latter-end of January last, about which time I syringed his eustachian tubes, by which he instantly heard his own voice, which he could not in the least before. I repeated the operation for three or four times, at a day or two distance from each other. He soon perceived a remarkable alteration for the better, together with this peculiar circumstance, that if spoke to as loud as was before necessary, the sound irritated his ear, causing a very painful titillation, or (as

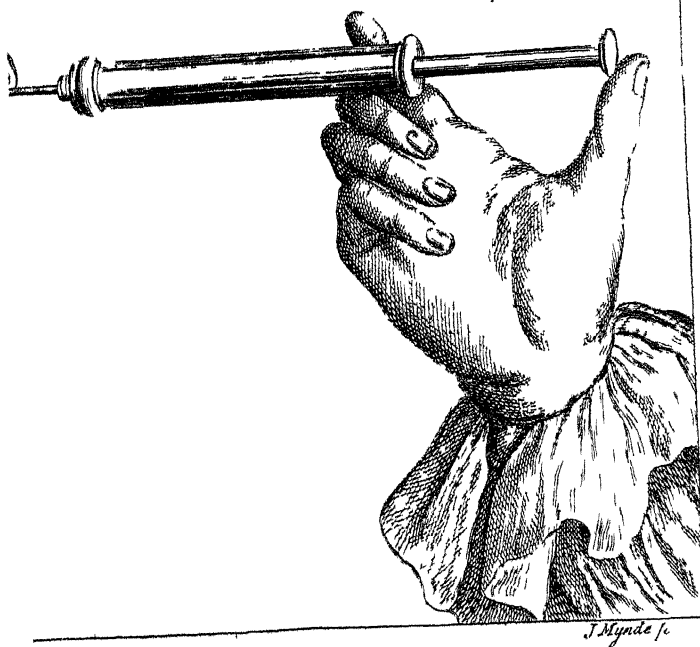
he himself termed it) a scratching in his ear. The same thing happened when he spoke, nor could he distinguish what himself or others said, except the voice was many degrees softer than he had long been used to: he can now hear a middle-ton'd voice, and converse with others very tolerably, if the room be quiet, and free from noise. He formerly taught the learned languages, by which he acquired three hundred pounds; all which he has spent in fruitless endeavours to regain his hearing. He has been twice deeply salivated, and his head profusely sweated for a long time together, and has undergone several physical courses; but nothing ever procured him the least help, till this operation was used; and it is remarkable, that the disorder of his eyes disappear'd after the second time his ears had been injected. Thus five out of the six cases received more or less benefit from the operation; without which they must probably have ever remained as they were, hopeless, and destitute of help.

I have endeavoured to ascertain the symptoms that indicate an obstructed tube, but have not been able to do it with any degree of certainty; nor can I see the great utility of it, could it be done; for the only disorders of the ear, that at present admit of surgical helps, are those of the external meatus, ulcerated and swell'd tonsils, &c. all of which are generally visible; and when they are not the cause of deafness, little or nothing is ever attempted, the patient being left to shift for himself. But now another probable chance at least is given to the unhappy sufferer, and being the only one (*e. g.* the others either improper, or tried before without success).











cess), may be made use of without delay, or attendance to corroborating symptoms, at least till they render themselves more conspicuous and certain than I have hitherto been able to find them : and as the operation is not at all dangerous, it neither has, nor will, I believe, be thought painful by those who desire to recover their hearing.

*Explanation of the References in PLATE VI.*

- A*, The cavity of the cranium, containing the cerebrum.
- B*, Process of the dura mater, called tentorium.
- C*, Cavity containing the cerebellum.
- D*, Posterior process of the dura mater.
- E*, Transverse spinous process of the sphenoid bone.
- F*, Orbiter process of the frontal bone.
- G*, Frontal sinus.
- H*, Body of the sphenoid bone.
- I*, Spongy substance of its body.
- K*, Holes in the sphenoid bone, through which the 3d, 4th, 5th, and 6th pair of nerves pass out of the cranium.
- L*, Sphenoidal sinus.
- M*, Sella turcica, in which may be seen
- N*, The glandula pituitaria.
- O*, Cribriform lamella of the ethmoid bone.
- P*, Cellulæ ethmoideæ.
- Q*, Os spongiosum superius.
- R*, Os spongiosum inferius ; by its concave surface a groove is form'd which serves as a director to the pipe of the syringe, in its course to the tuba eustachiana.

*S*, Palatine

*S*, Palatine process of the os maxillare, whose upper surface is strictly a horizontal plane, and not convex, from behind forward, as is always, tho' very unjustly, painted.

*T*, The orifice of the tuba eustachiana, whose posterior edge

*V*, is cartilaginous and prominent, forming a resistance to the extremity of the instrument, when push'd against it.

*W*, The upper and lateral part of the fauces.

*X*, Velum pendulum palati.

*Y*, Part of the foramen magnum occipitis.

*Z*, The inflected extremity of the syringe in the orifice of the tuba eustachiana.

XXXVI. *Tentamen Chemicum de Calcis vivæ  
Actione in Salem volatilem alcalinum.  
Auctore Johanne Alberto Schloffer, Ultra-  
jectino, M. D.*

Read June 5, 1755. **P**ostquam chemicorum solertia salium alcalinorum volatilium in genere omnium, e quacunque re artis vel naturæ viribus productorum, veras dotes atque characteres invenerat, constitit, salia hæcce diverso nomine a corpore, unde separata fuerant, petito, insignita, et ab imperitis ad hæc usque tempora haud raro pro diversis habita, quoties probe depurata fuit, omnia eisdem præcise gaudere dotibus, nullamque amplius inter se admittere differentiam. Ast simul artis chemicæ seduli observarunt, calcem (ut aiunt) vivam sali huic volatili alcalino

alcalino mistam, illum sic mutare, ut dum dotes generis alcalini volatilis quasdam fervet, plures amittat, quarum jactura necessitatem præbuit, sic mutatum falsum a vero sale alcalino volatili distinguere et specifico nomine designare, quod a variis varium datum fuit: exemplo sit nomen spiritus ignei ab immortalis Boerhaavio datum, dum igneam vim nescio quandam e calce viva falsi ipsi communicari credidit. Quum autem hæc denominatio, si igneas quasi dotes hujus sic a calce viva mutati falsi respiciamus, minus impropria videtur, me eadem in sequentibus usurum fore moneo.

Ipsius calcis vivæ origo, dotes, veraque natura, sat diu in obscuro latuere; et quamvis celeberrimus Hofmannus plurimas ejus dotes primus omnium experimentis rite descriptas nobis reliquerit, nihilominus quoad veram ejus naturam, agendique modum in fixa volatiliaque falsa alcalina, experimentis vel et hypotheseis suis in insignes errores inductus fuisse videtur (a); “ volatile quippe, penetrans, subtile et igneæ  
“ quasi naturæ (ut ait) principium calcis vivæ tribuit, illamque aqua solutam totam evaporare, ne  
“ particula quidem fixæ materiæ relicta,” audacter affirmat; dum indubitata infra citandorum illustrium in arte virorum experimenta assertis hisce falsius nihil haberi posse demonstrant. Magnus imo (b) Boerhaavius, qui in scriptis suis chemicis quidquid suo tempore veritatis notam acquisiverat optimam in for-

(a) Vide Hofmanni *Observat. physico-chymic.* lib. ii. obs. x. & xi.

(b) Boerhaavii *Chemia*, tom. 2. pag. 65. et in toto processu,

mam redegit, quum fingularem calcis vivæ in alcalina fixa æque ac volatilia falia virtutem, quam explicare nescibat, igni attribuit, et igneam inde vocat, supracitata Hofmanni auctoritate potius, quam propria sua usus fuisse, et ipsum quidem nomen spiritus ignei sic ab eo derivasse videtur. Illustris certe Hofmannus primus inter chemicos fuit, eratque tunc temporis unicus, qui veras dotes atque characteres spiritus ignei per experimenta stabiliverat, omnesque ita enumeraverat, ut quibus et quantum cum vero fale volatili alcalino conveniat, vel ab eo differat, optime descripserit; sed falsæ suæ hypothefi, quam de natura calcis vivæ conceperat, tantopere indulfit, ut experimenta negligens, spiritus ignei ortum et indolem per eam explicari debere non dubitaverit. Post Hofmannum et Boerhaavium vero a celeberrimis Europæ chemicis recentioribus (*c*), Pott, Duhamel (*d*), Macquer, Malouin, et hoc in regno ab (*e*) Alstono, Whyttioque

(*c*) Pott. *Lithogeognofia*, Parisiis Gallice edita, A° 1753. tom 1. pag. 11. ufque ad pag. 50.

(*d*) Trium horum insignium inter Gallos chemicorum experimenta actis Academiæ Regiæ Scientiarum Parisinæ inserta prostant; sed videri poterunt *Commentarii de rebus in scientia naturali et medicina gestis* (*Lipsiæ qui eduntur*). Volum. 2di partis 2dæ, pag. 204. ad 207. et ibidem a pag. 229. ad 232.— Ut et *Macquer Elements de Chemie*, tom. 1. pag. 66. et tom. 3. pag. 547.

(*e*) Celebrium virorum *Alstoni, Whyttiique Dissertationes medico-chemicæ de calcis vivæ natura, usque summo lithontripico*, ambæ in quinto volumine Actorum Edinburgensium reperiuntur; sed nuperrime, ob lites quasdam inter eruditos hosce viros ortas, novam quisque suæ dissertationis dedit editionem, ubi in additis notulis acriter disputant, utrum præcipitatus e miscela falis alcalini fixi atque aquæ calcis vivæ pulvis originem suam sali, calcive debeat?  
Suas

Whyttioque instituta ad determinandam calcis vivæ veram indolem pulcherrima numerosaque experimenta mirum quantum luminis scientiæ chemicæ attulerunt, vixque ullum reliquū èrehiatum. Non ignoro equidem quæ præsertim de salium quorundam diverforum in calce viva præsentia adhucdum disputantur: sed quicunque mecum sine præjudiciis omnium, quos supra nominavi, auctorum experimenta perlustraverit, et inter se contulerit, vix concludere dubitabit; “ Calcem vivam esse veram simplicem  
 “ terram alcalinam, ignis vi ad summam puritatem  
 “ deductam; hincque salia e calce viva extracta non  
 “ ad ejus compagem proprie pertinuisse, sed ab attractis ex atmosphæra variis acidis cum calce viva  
 “ ut vera terra alcalina unitis, ortum suum duxisse;  
 “ atque ideo, aquam calcis vivæ purissimam fortissimamque meram esse saturatissimam terræ hujus  
 “ alcalinæ solutionem ”. Illustris præsertim Pottius legatur in opere citato; quod si quidam e citatis auctoribus legissent, aliter et forte melius in suis experimentis processissent.

Calcis vivæ autem singularem in sale alcalinum volatilem virtutem, qua spiritus igneus Boerhaavii generatur, ingeniosus Duhamelius (quantum novi) primus omnium, et hucusque solus, ex arte chemica

Suas quisque profert rationes, sed utinam plures a folis experimentis petitas protulissent! Quæstionem certe hancce (quam minime parvo illorum experimentorum numero diremtam credo) e medio sublatam fuisse, omnes proculdubio lætassent chemici, simulque actio calcis vivæ in alcali volatile facilioris forte reddita fuisset explanationis. Sed quænam, quæso, est necessitas ortum ejus pulveris e solo sale, solave calce repetere? cur partim salinæ partim calcæreæ originis esse non posset?

G g

indagare,



indagare, riteque demonstrare tentavit, et pulchris experimentis detexit, salia alcalina volatilia, quoties ope salis alcalini fixi vel puræ cretæ e sale ammoniaco parantur, semper quandam ipsius alcali fixi cretæve partem sibi unire et in sublime secum ducere; dum e contra quoties calx viva ex eadem creta parata adhibetur, non modo nihil calcarei salii volatili alcalino jungi, sed etiam sublimationem spiritus ignei, nisi adsit aqua, nunquam fieri posse observabat. Multum porro doctus ille vir defudavit, ut probe discerneret modum gradusque unionis, quæ alcali fixi cretæve particulas sale volatili abreptas ipsi salii jungit. Sed nullo artificio, quamvis innumera tentaverit ingeniosissima, eas integre separare potuit: hinc concretam salis alcalini volatilis formam inde pendere putat; tandem ulterioribus tentaminibus invenit calcem vivam puram repetita salis alcalini volatilis admittance et destillatione pinguedinem quandam contrahere, atque spiritus ignei parvam copiam de multa calce viva sic pingui reddita destillatam aliquid salini concreti recuperasse: unde concludit, calcem vivam, dum alcalino salii volatili concreto jungitur, particulas ejus oleoso pingues ab aliis salem constituentibus particulis separare, sibi que unire, indeque spiritus ignei originem dotemque pendere (a).

Sed eruditus Pietschius, postquam in initio suæ dissertationis (b), pluribus novis plane experimentis veram

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(a) Hæc aliquæ singularia plane cum ipsis salibus ammoniacis ac celeberrimum Duhamelio instituta experimenta legi poterunt in *Actis Academiæ regię Scientiarum Parisinæ*, Anni 1735. pag. 106. 414. et 483.

(b) Videatur pulcherrima *Pietschii Dissertatio Chemica de nitri generatione*, A° 1750. Berolini edita, postquam ea præmium ab Academia

ram nitri murarii indolem, dotesque descripserat, pag. 19. dicit: " Instillando spiritum salis armoniaci  
 " nitri murarii lixivio, nubeculae orientur, et post  
 " perfectam quietem, subtilis terra atroflava ad fun-  
 " dum vasis secedet: mediante hacce terra, sal vo-  
 " latilis, quem calx viva destruxit, corporeus reddi  
 " poterit." Hoc vero phaenomenon, aequae ac simi-  
 lis plane praecipitatio, quam ab alcali fixi solutione  
 nitri murarii lixivio admista, contingere observaverat,  
 magnam dabat doctissimo viro difficultatem, dum  
 rem ex ipsius nitri murarii textura et compositione  
 eruere voluit; quae mihi non nisi ex ipsissima super-  
 flua basis calcareae copia in lixivio salino soluta, et  
 aquam calcis constituyente, petenda videtur. Id vero,  
 dum haec Pietſchii verba legerem quasi subolfaciens,  
 percepi simul genus experimentorum a Pietſchio hic  
 indicatum (si nempe loco lixivii nitri murarii puram  
 calcis vivae aquam adhiberem) novum non modo  
 fore tanquam a Duhamelio plane neglectum, sed et  
 maximi forte momenti. Ex supra laudatis enim Al-  
 stoni Whyttiique opusculis neminem hodie latet  
 quantum humani generis intersit probe perspectos  
 habere effectus, modumque agendi verum calcis vivae  
 alcalinis salibus applicatae, quippe sine quibus egre-  
 gia illius virtus lithontriptica a viris hisce celeberrimis

Academia regia Scientiarum Berolinensi iustissime reportaverat: ille quippe vera acidum nitri constituentia principia illud artis ope parando, non tantum demonstrat, sed, quod summam totius Europae meretur attentionem, artificialem huiusce acidi ipsiusque veri nitri confectionem antea plane incognitam facilem adeo esse docuit, ut hodie Regis Borussiae iussu, muris hunc in finem, juxta Pietſchii principia, per totum fere regnum extractis, magna copia pareatur.

mis tanto labore atque studio in hominum inestimabile bonum stabilita, nullo modo intelligi et multo minus explicari posset, simulque per analogiam vel aliter a priori plura similem effectum possidentia remedia difficulter detegerentur. Credidi igitur pauca frequentia experimentula, indeque rite deductas veritates, aliquid ad promovendum nostrum de hac re intellectum posse contribuere; et dum mentio, quam ab (a) Alstono factam vidi, de præcipitatione quadam ab eo in miscela spiritus cornu cervi cum aqua calcis observata, quam dum explicare non potuerat, uti omnes ingenuos decet viros, ulterioris examinis causa chemicis proposuit, me quotidie de hisce cogitantem ulterius instigaret, doctissimus vir dominus Lucas, medicinæ doctor, quique chemiam summa laude et labore nunc hic Londini publice docet, ex integerrima sua amicitia, verus mihi auctor fuit tentamina, quæ animo volvens ipsi proponebam, statim instituendi, instructissimi sui laboratorii chemici usum dum peregrinanti mihi concessit non modo, sed et peritia sua veraque eruditione chemica omnem, quam desiderare potueram, opem tulit.

## S E C T. I.

Unius drachmæ salis alcalini volatilis e sale ammoniaco per alcali fixum parati concreti purissimique in aquæ pluvix destillatæ pauxillo solutionem confeci, eamque extemplo capaciiori nitido vasi vitreo immisi, ac statim duodecim uncias aquæ calcis vivæ (e lapi-

(a) Pagina mihi 21. Dissertationis suæ supra citatæ, edit. noviss.

dibus paratæ) purissimæ fortissimæque ipso momento, quo adhiberetur, filtratione a crustulis suis, q̄ as dum supra calcem stetit, contraxerat, liberatæ superfudi. Tuncque videre mihi l'cuit excolores pellucidissimosque hosce liquores, eodem, quo sese tetigere tempore, lacteam levem contraxisse opacitatem sensim magis auctam, donec integrum probe mixtorum volumen lactis instar albesceret, quando quoque odor volatilis alcalini salis multo acrior factus fuisse videbatur. Obturato tunc vasis ore post breve tempus minutos valde flocculos numerosissimos undique natantes observabam, qui non nisi lente ad fundum lateraque vasis secedebant; prout vero secessere, lactea liquoris periit opacitas, pristinaque rediit excolor pelluciditas. Tum de novo duodecim ejusdam aquæ calcis uncias eidem vasi eodemque modo instillavi, ac eadem perfecte phænomena contigisse vidi. Uno verbo novam toties aquæ calcis (notatam semper) affudi copiam, donec nulla amplius coloris vel pelluciditatis oriretur mutatio, curam gerens debitæ qualibet vice mistis liquidis concedendæ quietis, antequam novas aquæ calcis guttulas ad explorandum adderem; sicque ad plane perficiendam hac methodo præcipitationem illam, quinquaginta quatuor aquæ calcis uncias adhibui, ultimæque, quas addidi, quamvis valde levem, visibilem tamen adhuc quandam coloris inducebant mutationem: ast completo, quem dixi numero, liquoris illius guttula unciis aliquot aquæ calcis instillata, vel vice versa aquæ calcis guttula toti hujus liquoris immersa volumini, nullam ne vel minimam lactescentiæ, &c. notam præbebat. Quibus peractis forti vasis concussione flocculos, qui ad fundum lateraque vasis in omni puncto secesserant,

ferant, inde dimovere conatus fui, sed frustra; magna quippe eorum copia undique firmiter nexa remansit, adeo ut parvo bacillo ligneo omnem vasis internam superficiem ab iis quantum potui liberare debuerim; tuncque sine mora omnem hunc liquorem cum cunctis hisce flocculis cono chartaceo puro siccoque (cujus pondus notaveram), in loco minime pulverulento infudi, et toties transcolare feci, donec omnibus flocculis intra conum collectis, liquor totus quantus pellucidus transierat, qui tunc sequentia præbuit phænomena.

## S E C T. 2.

1. Saporem habebat blandum, quique ne vel minimum salini indicium dabat gustui, sed debilissime potius calcis aquam indicabat.

2. Odore gaudebat spiritus ignei acerrimo.

3. Syrupo violarum mixtus vix ullam viriditatem ipsi conciliavit.

4. Cum acidi vitriolici puri fortissimique parce diluti guttula fere nullam effervescentiæ notam dabat.

5. Nullusque in eo fiebat visibilis ab alcalini fixi salis solutione effectus.

6. Retortulæ vitreæ nitidæ immixtus, et appenso excipulo, rimisque rite luto obfignatis, leni calore inde destillatus in omni puncto colli retortæ laterumque excipuli guttas aqueas roridas oriri, nullibi vero strias formari vidi, neque vel minimum concreti salis ullibi mihi vestigium apparuit.

7. Collectus ab illa destillatione in excipulo liquor acerrimo suo urente quasi spiritus ignei odore nares violenter feriebat; et acido vitrioli optimo dilutoque placidissime sese commiscuit.

8. In retortula vero residuum post destillationem hancce liquidum

- a.* Leviter flavescentem contraxerat colorem :
- b.* Odorem spiritus ignei sed debilissimum adhuc servaverat :
- c.* Pristinumque debilem aquæ calcis saporem.
- d.* Immissa vero salis alcalini volatilis concreti parca copia nulla lactescentia vel præcipitatio secuta fuit, sed sal æque ac in simplici aqua limpide dissolvebatur.
- e.* Nulla quamvis cuticula, crystallulæ, pulvisve, ad ejus superficiem, vel retortæ fundum laterave apparuerint, tres ejus uncias e vasculo vitreo amplo evaporare feci; neque tunc vel minimam vidi cuticulam, cristallulasve: hinc ad perfectam usque ficcitatem evaporationem ejus protraxi, qua nihil præter tenuissimam fulvo grisei coloris crustulam ad solum vasculi fundum confectus fui, quæ vero omne examen tenuitate sua effugiebat.

### S E C T. 3.

1. Conus chartaceus, qui omni transmissio liquore folos supra descriptos flocculos retinuerat, probe cauteque iterum exsiccatas drachmas duas tredecimque grana ponderabat, dum antequam filtrationi adhiberetur, pondus unius drachmæ et septem granorum fuerat.

2. Siccissimi nunc horum flocculorum pulveris, (qui tenuis atque levissimus erat) colorem plane niveum deprehendi.

3. Saporem offendi nullum, neque ullum odorem.

4. Alkali

4. Alkali fixi saturatissima solutio pulveris hujus tantillo superfusa nullum effectum sensibilem excitavit.

5. Quamvis dilutum acidum vitrioli pulverem hunc magno impetu aggreditur, atque parte quadam solvit; solutum vero sale fixo alcalino addito præcipitem denuo dat.

6. Pulvisculi hujusce drachma integra parvo nitidoque crucibulo fuit immissa, et ad perfectam ignitionem gradatim aucto igne deducta, pulveris tamen formam retinuit, et statim ac leviter refrigeratus fuit: ad stateram quum exploraretur, fere sexdecim grana sui ponderis amisisse patuit, gustumque acquisiverat, verum illum calcis (vivæ dictæ), qui quasi igneus videtur: imo simplex destillata aqua pluvia paucis ejus sic calcinati pulveris granis affusa citissima in veram aquam calcis vivæ mutata fuit docente sapore, illiusque in additum salem alcalinum volatilem effectum præcipitatorio.

7. Eiusdem puri pulveris scrupulus unus intra parvum nitidamque retortulam vitream igne per gradus ad summum usque deducto, quamvis diutissime ustulatus fuerit, una alterave aqueæ humiditatis rorida excepta guttula, nihil visui vel odori percipiendum præbuit; neque pulvis ille habitum suum hoc in experimento ullo modo vel respectu mutasse visus fuit.

8. Illius iterum pulveris ficci purique scrupulum integrum dimidiæ unciae aquæ simplicis destillatæ, (quæque cum aqua calcis vivæ optima nullam faciebat lactescēntiam) immisi, et sic intra vasculum purum clausum probe calefeci, repetitis vicibus pulverem vasis concussione cum aqua commiscendo; quibus factis clausum vasculum per 24 horas in quietum

tum locum reposui, postea vero illam prudenter de pulvere decantavi, et

- a. Saporem ac odorem ejus immutatos deprehendi.
- b. Mixta cum aqua calcis vivæ fortissima nullam omnino lactescentiæ præcipitationisve præbuit notam.
- c. Acido vitrioli diluto mixta minima ebullitionis notula non apparuit.
- d. Syrupi violarum colorem nullatenus mutare valuit.

9. Spiritus ignei optimi (postquam rite paratus fuerat de nova calce viva rectificati) dimidiam unciam retortulæ vitreæ, cui pulveris nostri drachma immissa fuerat, infudi, et appenso vitreo puroque excipulo rimis probe luto obfignatis, lenem primo calorem digestorium applicui; tandem vero omnem spiritum in vas excipiens destillare feci: destillatio pro maxima parte guttarum roridarum forma processit, paucis hinc inde apparentibus striis, neque ullæ crystallinæ particulæ sub destillatione apparuere, sed interrupta fere media destillatione refrigeratoque plane apparatu, hinc inde parvas ad recipientis vasis retortæque colli latera ac in liquorum superficie crystallos observavi.

2. Spiritus postquam omnis in excipulum transierat, soluto apparatu, non adeo acrem urentemve quam antea spiravit odorem.

3. Acido vitrioli parvum diluto cum miscebatur, strepitum, qui a spiritu igneo fieri solet, fere nullum excitavit, sed satis visibilem quamvis levem ebulli-



tionem præstitit, odoremque exhalantem percepi omnino similem illi, qui sale volatili alcalino vero et acido vitrioli ad confectionem salis secreti Glauberi mixtis oriri solet.

4. Aqua calcis vivæ optima huic liquori instillata conspicuam valde lactescentiam atque præcipitationem præstitit, quamvis non adeo fortem, ac verus purusque sal alcalinus volatilis fecisset.

5. Acido salis marini liquori eidem admixto, levis orta fuit ebullitio, et prudenter evaporata hac miscela parvas paucas accepi cristallos, spiculata sua tenuitate eas salis ammoniaci probe referentes, atque statim ac alcalini fixi salis solutio eis superfundebatur, verum salis alcalini volatilis odorem exhalantes.

6. Residuus intra retortam hærens pulvis subtilissimus undique lateribus ebulliando sic aspersus fuerat, ut sine insigni jactura inde separari ac colligi non potuisset: hinc pondus ejus non examinavi, ast moles fere pristina videbatur, sicuti cæteras quoque ejus dotes nullo modo mutatas deprehendere valui.

#### S E C T. 4.

Oporteret nunc cunctas, quas experimenta mea præbere possunt, sequelas rite deducere et corollariorum forma justo enarrare ordine. Ne vero nimium in volumen cresceret dissertatio, succincte potius proponam, quomodo hæc phænomena contingere, et ex veris scientiæ chemicæ principiis (quousque mihi notis) explicari posse videantur. En hæc mea est sententia!

“ Ipso momento, quo calcis vivæ particulæ, quæ  
“ in aqua calcis solutæ hærent, veri salis alcalini vo-

“ latilis particulas tangunt, lactefcentia fit, et calcis  
 “ vivæ particularum præcipitatio sequitur. Simul vero  
 “ compages salis volatilis alcali in ipsa sua textura  
 “ dissolvitur, acidis ejus, parciq̃ue phlogisti sui, par-  
 “ ticularis magna parte per præcipitatam calcem at-  
 “ tractis et cum ea coeuntibus; dum reliqua salis  
 “ alcalini volatilis pars junctura hacce longe acrior, et  
 “ amisso quasi corpore suo vere spirituosâ reddita  
 “ spiritum igneum Boerhaavii verum constituit; qui  
 “ igitur in eo tantum a vero sale alcalino volatili dif-  
 “ fert, quod particulis hisce acidi atque phlogisti sui  
 “ maxima parte privatus fuerit, quibus debite re-  
 “ stauratis spiritus igneus verum in salem pristinum  
 “ totus quantus regenerabitur.”— Duhamelii certe  
 meaque quantulacunque fuerint experimenta ultimum  
 hoc assertum valde probabile reddidisse mihi viden-  
 tur: ast fateor restat tentandum, “ An purus spiritus  
 “ igneus cum subtili quodam acido parcaque tenuis-  
 “ simi phlogisti copia digestus, aliove modo copu-  
 “ latus, veri concreti salis alcalini volatilis dotes om-  
 “ nes recuperare valebit?” Eritque hæcce sola vera  
 methodus demonstrandi naturam et quantitatem ea-  
 rum particularum, quæ per calcem vivam a sale vo-  
 latili alcalino separatæ illum in spiritum igneum mu-  
 tant, quasq̃ue Duhamelius primus omnium detexit,  
 sed mere phlogisticas esse, et a contentis in calce viva  
 ignis veri particulis (quas cum Boerhaavio retinuerat)  
 absorberi credidit; dum calx viva ut mera pura sed  
 sicca, fixa fortisque terra alcalina considerari, et ex  
 hac notione illa ejus in alcalinum volatilem salem  
 actio derivanda videtur. Ultimis quippe suis chemi-  
 cis laboribus nostri sæculi chemicorum princeps Pot-  
 tius

tius (a), jamjam docuit; salem alcalinum volatilem, ex acidi salis marini, terræ subtilissimæ, parcissimique phlogisti intima unione conflatum esse; fixum, dum salem alcalinum e variis talibus principiis sub diversis conditionibus ac proportionibus artis ope confici dum novimus. Quum ergo calx viva est vera terra ista alcalina, cujus debita cum aliis illis elementis unio salem alcalinum fixum ipsum præbet, quid quæso obstat quo minus concluderem, spiritum igneum e vero sale volatili alcalino calcis vivæ ope fieri, quia acidi, ut et phlogisti, quædam particulæ majori vi ab alcalina hacce fixa terra attrahuntur, quam ab ipsis terreis salis alcalini volatilis particulis? Certe non obstat, quæ valde paradoxa videri posset, spiritus ignei puri fortisque, cum præcipitata per volatilem alcalinum salem calce, digesti parva quam descripsi regeneratio: puritas enim ac fortitudo dephlegmatissimi spiritus ignei facile denuo quandam copiam, si non omnem, prius attractarum a calce viva particularum liberare et recuperare valebit. E contrario ex hac theoria cuilibet clare patet, cur spiritus igneus purissimus aquæ calcis vivæ quamvis fortissimæ mixtus nullam prorsus lactescentiam, aliumve ullum visibilem præstet effectum, quod experimentis propriis didici. — Sed sufficienter differui, et finem imponere festinabo: liceat mihi tantummodo sequentes omnibus in genere chemicis proponere questiones.

1. Lithontriptica calcis vivæ virtus a celeberrimis viris supra laudatis firmiter adeo stabilita, estne soli huic ejus actioni, qua sales alcalinos volatiles destruit, adscribenda?

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(a) *Pott Lithoæognosie*, editionis Gallicæ, vol. 1. pagina mihi, 63.

2. Acidi falis nativi urinæ humanæ stupenda proprietas (*b*), quam dum verum falem alcalinum volatilem sibi unit, et cum eo in falem neutrum nativo urinæ humanæ fali pristino perfecte similem regeneratur) idem ille volatilis alcalinus fal in spiritum igneum sincerissimum mutatus deprehenditur, æque ac si calx viva adhibita fuisset, estne quæso ex iisdem explicanda principiis, quibus ad explicandam hanc calcis vivæ virtutem supra usus fui?

3. Cur igitur non tentaremus acidum hocce falis urinæ nostræ nativi nativum et blandum quod est, ac in parcissima solvitur aqua, calculo laborantibus debita dosi deglutendum dare, vel et in aqua solutum vesicæ calculum continenti injicere, eodem scopo lithontriptico, quo calcis vivæ aquam porrigimus? ab actionum quippe chemicarum similitudine ad virtutes medicas conclusum facilis satisque tuta datur analogia.

Scripti Londini, a. d.  
10 Maii, A° 1755.

Johan. Albertus Schloffer,  
*Ultræjectinus, M. D.*

(*b*) Hanc veritatem (quam huc usque ex veris principiis chemi-  
cis intelligere non potui) jam ante 2, annos descriperam et expe-  
rimentis demonstraveram in fine *Dissertationæ meæ inauguralis de*  
*fale urinæ humanæ nativo*, A° 1753. Lugduni Batavorum editæ:

XXXVII. *An Account of a very remarkable Case of a Boy, who, notwithstanding that a considerable Part of his Intestines were forced out by the Fall of a Cart upon him, and afterwards cut off, recovered, and continues well.*

Read June 12, 1755. **O**N the third of January 1755, I was called to John, the son of Lancelot Watts (a day-labourer, living at Brunsted), a servant-boy to Mr. Pile, a farmer at Westwick, near North-Walsham, Norfolk, aged thirteen years. He was overturned in a cart, and thrown flat on his face, with the round, or edge of one side of the cart, (bottom upwards) whelmed across his loins, the upper part of the body lying beyond the wheel at right angles. In this helpless condition he continued some time, and was found with a very large portion of the intestines forced out at the anus, with part of the mesentery (and some loose pieces of fat, which I took to be part of the omentum), hanging down below the hams double, like the reins of a bridle, very much distended and inflamed. He had a continual nausea, and violent reachings to vomit, and threw up every thing he took. The pain of the stomach and bowels was exquisite, attended with convulsions; his pulse low and quick; and frequently he fell into cold sweats. After using an emollient and spirituous fomentation, I reduced the parts, though to no purpose: the vomiting immediately returned, and forced them out again. Next day the fever increased, the nausea and reachings to vomit continued, the  
parts

parts appeared livid and black, with all signs of a mortification. On the third day the mortification increasing, I cut off the intestine, with the mesentery, close to the anus. He had had no stool from the time of the accident, but soon after the operation there was a very large discharge of blackish and extremely offensive fæces, which continued several days, lessening by degrees. He soon grew easy, and the nausea and vomiting abated. I gave him *Tinct. Cort. Peruv. simpl.* twice a day; and, as he complained at times of griping pains, he took now and then *Tinct. Rhubarb. vinos.* and has recovered a good state of health. For some time he had six or seven, or more stools in a day; at present commonly three or four, all loose, which come soon after eating; and frequently he is obliged to hurry out to ease himself, during his meals.

I have three times lately tried, if I could discover a passage through the coats of the rectum, with my finger, and I think I have always felt an opening, just above the sphincter, towards the spine; the circumference of which was full, and protuberated, seemingly as large as my finger, the lower edge of which was harder than the rest: he complained of pain, when I pressed the upper part.

The intestine cut off measured fifty seven inches, by a string applied to the outer surface.

On the 7th of May the boy came walking from Brunsted to North-Walsham (seven miles), and dined with me, was perfectly well, and walked back again that afternoon.

North-Walsham, Norfolk,  
May 28, 1755.

John Nedham.

Witness, E. Brooke, Surgeon.

XXXVIII.

XXXVIII. *An Account of some Experiments on the Sensibility and Irritability of the several Parts of Animals; in a Letter from Richard Brocklesby, M. D. F. R. S. to the Reverend Thomas Birch, D. D. Secr. R. S.*

Dear Sir,

Read June 19,  
1755.

A New doctrine some time since propagated by a most faithful observer of the animal oeconomy, Dr. Haller, the illustrious commentator on Boerhaave's institutions, prevailed on me to do violence to my own temper, whilst I was to be witness to the success of most of the experiments, which have been mentioned in his *Dissertation upon the Irritability of animal Fibres*, as it deserves to be distinguished from sensibility.

Nothing short of the general importance of this inquiry could have induced me to be spectator of such cruelties, as indeed I have been within this month past. But as I shall anticipate partly the conclusions, that every gentleman conversant in such matters will draw from what follows afterwards, I am confident, that, however cruel my experiments may at first hearing sound in your ears, yet in the end you will conclude, that the pain and misery by me caused even to the victims of this subject, were to be regarded much less than what happens every day in Smithfield to twenty oxen and sheep, by cutting off their tails, and other parts of the skin, and driving them miles afterwards.

But

But to avoid, as much as might be, unnecessary privation of animal life, I selected at various times many objects, more than I shall, for a particular reason, at present relate; but most of them immediately devoted to death by the butcher for the use of the market, if I had not been present.

The first experiment I propose to relate, was made by cutting four inches of a young lamb's skin, which covered the great tendon of the hinder leg, and is known to anatomists by name of the *Tendo Achillis*. This of course caused violent struggles, and other marks of the injury felt; and on touching the extremity of the skin, whilst united to other parts of the animal, it cried loud, urined, and voided its excrement, when I poured diluted spirit of vitriol upon the edges of the skin, that were fixed to the contiguous parts; but did not express much pain by irritating the raised skin, at the farthest extremity of its separation, by an affusion of diluted spirit of vitriol. Nearer however to the fixed parts underneath, the sensation in the raised part of the skin continued much longer.

I then made the butcher cut into the tendon half way, and divide it upwards more than two inches, and attentively stood over the animal, to watch his motions, and discover if there was any apparent pain: but whilst that was doing, I could discern none, nor any marks of sensation in the animal, whilst I handled and pulled the cut tendon, nor yet any on touching it with dulcified spirit of nitre, and sharp acid spirit of vitriol; and what yet surprised me more, was to find the creature as insensible upon the tendon, as if it was a mere piece of glue, when I put a strong



muria of sea-salt and nitre all over it; and after a very few minutes I laid the raised part of the tendon in its natural direction, upon the correspondent fixed part, and they were both exactly congruous; so that the loose part had not contracted itself, nor was at all shorter, after these repeated trials, than its correspondent fixed part. I then put the creature on its legs, to see whether it had suffered so much, that it could not use the leg; but it was found to walk, though favouring greatly that side where so much had been done; however, it walked fairly on all its legs. After about five minutes torment, the butcher ended all its pains, and I performed the same processes on a sheep just destined to be slaughtered, in which I found all the appearances as above-mentioned.

I was induced to make two other very cruel experiments on different animals, by laying bare their patella's of the knees: having cut off all the skin round about, I then pricked and touched with the afore-mentioned escharotics the capsular ligaments of these joints, without discovering any tokens of pain thereby occasioned; but as soon as the sharp fluids had spread over the surface, so as to reach the extremity of the skin, the creature underwent as much pain as cutting before had caused.

I desired the butcher to take off as much skin from the forehead, as was necessary to perform the operation of the trepan; and before I began to apply the instrument to the sheep's forehead, I vellicated the pericranium with the end of a knife, but could not observe the membrane sensible, or thereby thrown into contractions; and when the operation was over, and the bone taken from the subjacent dura mater,

I poured

I poured on this membrane dulcified spirit of nitre, and diluted spirit of vitriol, and powdered common salt, but without perceiving any agitations whatsoever, brought on by these substances acting upon these living parts; though in some creatures I am dubious, whether sea-salt and nitre in powder did not create some sense, though no manifest contractions of the dura mater.

But every muscular part, which I cut while the animals were alive, discovered little sensibility of pain, though great propensity to irregular spasms of the fibres: and the muscles upon the thorax, and especially the *carneæ columnæ* of the heart, retained irritability last of all other muscular parts, even till long after the animal's expiration.

I laid the pungent liquors and salts, as above, upon various parts of the animal, yet alive; as upon the fat, cellular membrane of the neck, leg, and other parts within the skin, the liver, pancreas and spleen, and could not find them endowed either with remarkable sensibility or irritability; nor had the bladder any remarkable symptoms of irritability, farther than might be occasioned by its muscular fibres; though the well-known symptoms of the calculus shew its great sensibility.

I tried the effects of a strong aqueous solution of opium upon the irritated parts of muscular fibres, but could not perceive an opiate manifestly to compose these spastic motions of the parts, as Haller alleges they do: though in some trials I fancied there were grounds for such a conclusion. However this is no argument against the internal use of opiates, where the solids are greatly irritated.

I must add one more experiment, which I made upon the intestines of a lamb: after I had taken them from the carcase, I poured diluted spirit of vitriol on them, as well as several other pungent substances; and upon the touch of all of them, the intestines renewed their contraction, which before had totally ceased, and surprised me with a motion almost as strong as is found in the process of chylicification; and this continued till the external cold had indurated and stiffened the fatty membrane of the omentum.

These were some of many experiments of a like nature, which the importance of these facts in daily practice of medicine required to ascertain, or reject; and, from the result of my repeated trials, I am induced to coincide with most of the conclusions drawn by Drs. Haller, Castell, and Zimmerman; that no part is sensible but the nerves only, and that some parts are irritable without sensibility accompanying them in any great degree; whilst others are altogether without sense, at the same time that they are incapable of being irritated at all.

Thus, Sir, according to your desire expressed in yesterday's conversation, I have communicated, for the amusement of some gentlemen of the Royal Society, a candid narrative of many of Dr Haller's experiments, as repeated by me: and whether I shall, by prosecuting the subject still farther, be able fairly to make out, that irritability, as it is distinguished from sensibility, depends upon a series of nerves different from such, as serve either for voluntary motion and sensation, at present is impossible to say. But whatever shall be my future conclusions, I will establish nothing hypothetical, but endeavour by fair deductions

deductions to approach towards truth, as near as the abstruse nature of the subject will permit: And as I think I have actually found some variation from the common practice in rheumatisms, built upon the established fact of great irritability in the muscular fibres, succeed, to the relief of suffering patients, I cannot dismiss this subject, without relating, that only with gentle and continued frictions on the pained rheumatic parts with common salad oil, two poor patients, who lately applied for my advice in obstinate rheumatisms, were, by thus relaxing the crispation of the solids, surprisngly relieved, without any farther medicine. So that after bleeding, where it is indicated, which above all things I find abates irritability, it may deserve to be tried, how far animal oils, applied by friction long continued to the aggrieved parts, both in the gout, rheumatism, and other painful diseases, will ease the tortures, without repelling or obstructing the matter, which nature is labouring to throw off. But I forbear to enlarge, as the experiments I have hitherto made upon the subject of irritability, are scarce sufficient to obtain what Lord Bacon calls the *Vindemiatio prima* in this science. When I receive sufficient information to be convinced within myself, I shall not be wanting to communicate what may tend to advance this branch of natural knowledge, and to promote a true theory of diseases, upon which all rational practice must be established. I have now obeyed your friendly commands; and that the gentlemen and you may not think me tedious, I conclude with assuring you, that I am, Sir,

Your most obedient humble servant,

New Broad-street,  
June 19, 1755.

Richard Brocklesby.

XXXIX. *An Account of Worms in Animal Bodies. In a Letter to Tho. Birch, D. D. Secr. R. S. from Frank Nicholls, M. D. Med. Reg. and F. R. S.*

S I R,

Read Nov. 6,  
1755.

**A**MONG the primary causes of destruction to animal bodies, it seems probable, that worms are more frequently concern'd than is generally imagined. I have often observed worms in different parts of the body, which, I should think, could not exist without great disturbance to the oeconomy, and perhaps at last must be fatal to the animal.

Fish are, to appearance, more subject to worms than other animals: the cod often shews small slender worms, coil'd up like snakes, on the surface of its liver: and the bley in our Thames, about the month of July, is often distress'd by a long flat worm, which, by possessing and eating its liver, prevents the fish from compressing itself to that specific gravity, which is necessary for its quiet continuance under the water; so that it is obliged to skip about upon the surface of the water, till it becomes a prey to its foes, or dies suffocated, by its being so often out of water, and deprived of that action of the water, which is analogous to the force of the air to us in breathing.

Among the many cases, which I have seen, two seem to deserve our particular attention, as well because they are greatly prejudicial to the farmer, as  
because,

because, when generally known, they may possibly lead to a method of successful cure.

The first of these is a species of dropfy, incident to bullocks and sheep. In opening these animals, when dead of this rot, the liver is always found affected. A small flat worm, resembling a sole (and often many of them), is found in the gall-duct, by the butchers term'd flocks. It is the property of this worm, that it always builds a wall of stone for its defence; which wall is ramified like the gall-duct, within which it is formed. This stony tube (when completed) blocks up the gall-duct, and stops the passage of the gall; which thereby furcharging the duct, and dilating the orifices of the lymphatics, returns again into the blood, and gives the yellow teint to the eyes, which is the first symptom of this disease, and generally precedes the loss of flesh, and the swelling of the belly. It seems probable, that whatever can increase the acrimony of the bile, must be useful in preventing this disease; but when the stony pipe is form'd, no method seems capable of promoting its discharge, or dissolution.

The other case is termed the husk, and is a disease, to which bullocks are very subject, while young; for it rarely affects those of more than a year old. The creature is seized with a short dry cough, by which he is perpetually teized; in consequence of which he wastes in flesh, and grows weaker and weaker till he dies.

Upon opening the lungs of a calf dead of this distemper, I found the windpipe, and its branches, loaded with small taper worms of about two inches long, which were crawling about, though the animal had  
been

been dead many hours ; and the farmer assured me, that they always found these worms in this distemper, and knew of no method of cure.

I should have great hopes however, that fumigations, either with mercurials, as cinnabar, or with fætid, as tobacco, properly used, might prove of great service.

You have herewith the two species of worms, (See PLATE VII. *Fig. I.*\*) in order to be lodged in the repository of the Society, if it be judged requisite. I am, Sir, with the greatest respect to you, and the Society,

Lincoln's-Inn-Fields,  
Nov. 6, 1755.

Your most humble servant,

Frank. Nicholls.

*XL. An Account of some remarkable Insects of the Polype kind, found in the Waters near Bruffels in Flanders. In a Letter to Thomas Birch, D. D. Secret. R. S. from T. Brady, M. D. Physician to his Highness Prince Charles of Lorraine.*

S I R,

Read Nov. 6,  
1755.

**T**HE plant, of which I send you the inclosed draught, is found in summer-time, in all sorts of ditch or stagnant waters : its colour is white, and its transparent body, when seen with the naked eye, is in length betwixt one  
and





*Fig. 1. p. 248.*



*Fig. 1. p. 249.*



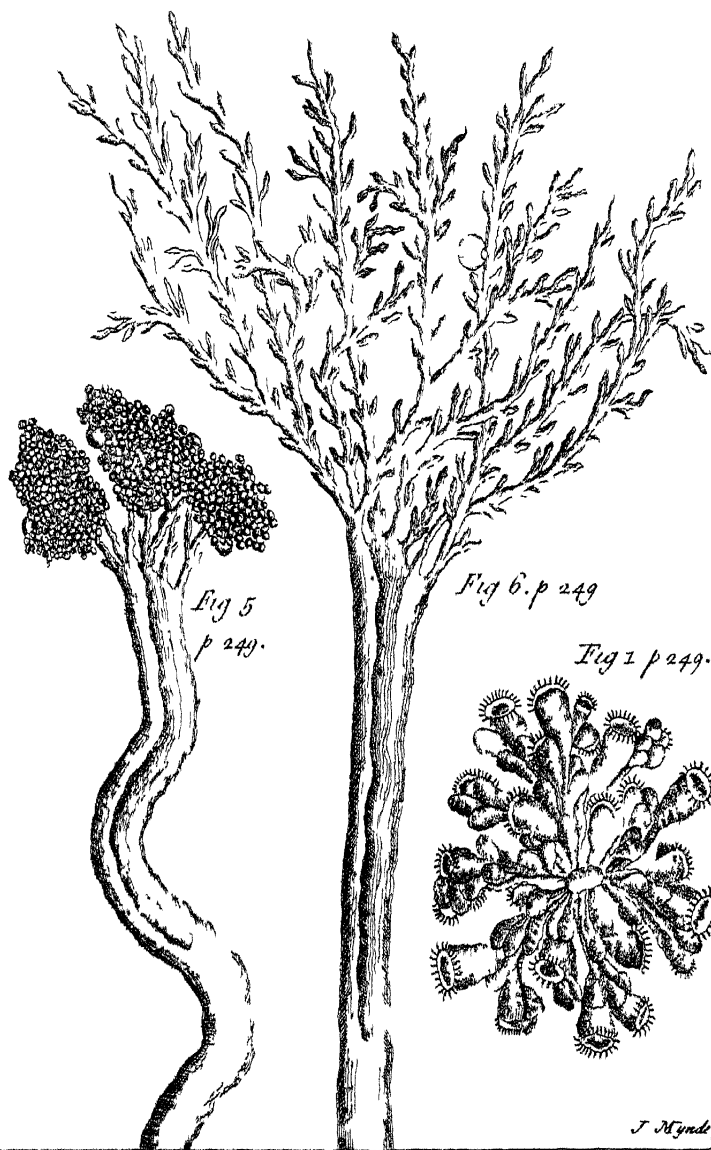
*Fig. 4.  
p. 249.*



*Fig. 3. p. 249.*



*Fig. 2.  
p. 249*



*Fig 5*  
*p 249.*

*Fig 6. p 249*

*Fig 1 p 249.*



and an half and two lines, but when view'd with a good microscope, whose focus is about eight lines, it appears as you see in **PLATE VII.** *Fig. 1.* with leaves, branches, and fruit, and indued with such sensibility, that at the least noise made in the room, or upon any thing's touching the table where your microscope stands, or the water in which it lies, it contracts itself with such activity and swiftness, that the eye cannot follow it in that motion, till it reduces itself into the shape you see it twisted in *Fig. 2.* The extension or dilation goes slower, and requires about half a minute before it comes to the form you see it in, like a grape, in *Fig. 5.* It can live in its own standing-water for eight or ten days, and then looks as you see it in *Fig. 6.* as most trees do in winter-time. It is remarkable, that the leaves, which are like bells, live some time after they fall, and retain that faculty of contraction and dilatation; and when you view them with our great magnifier, whose focus is about two lines, it appears as in *Fig. 4.* The trunk is as you see it in *Fig. 3.* The number of its branches are undetermin'd, but commonly found to be betwixt six and twelve. We have not tried, if it does not regenerate, when cut, like polypes: But one can see a vast difference betwixt it and the *Polype à bouquet*, of which Mr. Trembley makes mention. The other curious insect represented in *Fig. 1.* is found in the same standing-waters with the plant, and is seen with the naked eye, like a little flat round leaf, whose diameter is about one line and an half; but when put in a microscope, it shews a circle surrounded with crown'd heads, tied by small thin tails to a common centre, from whence

they advance towards the circumference, where they turn like a wheel, with a great deal of vivacity and swiftness, till they cause a kind of a vortex, into which you will see all smaller insects or bodies either attracted or driven, which probably serve as nourishment for those little crown'd things, who in all appearance are, as well as the plant, a sort of insects of prey, that live on smaller creatures. When one of those little heads has wheel'd a while, it rests, and another turns out; and sometimes you will see three or four wheeling at a time. We have seen last year some much more regular, that formed an orderly circle, with their crowns to the circumference, and their thin bodies like so many radius's join'd to the centre. Their motion is all strait towards the edge of the circle, and never to the right and left, as if every head had its proper limits to act upon. One Symoy, an optique instrument-maker, found the first of those insects as he was searching for polypes; which gave me since an occasion of examining ditch-water, where I not only found the plant, but the crown'd insect, and a good many other surprising little things, of all shapes and fashions, of which I will take the liberty to send you an account.

Mr. Mitchell, the British minister for the congress at Brussels, saw those curious insects, and observed, which we did not take notice of before, that the fruit of the plant, which resembles an orange, has a kind of a chain about it, that turns as the crown does in the other insect. He took notice also, that the trunk or stock of the plant was its gut, or stomach; for he saw, that something descended through it, as it  
were

were through a gut. Besides it has no support of any fix'd point, but is always swimming in the ditch-water, and shews no great local motion. We saw other insects preying upon it, which resemble small hogs, and are excessive busy in eating of its leaves, which are probably the cause of its looking so bleak and withered when dead. I remain,

S I R,

Your most humble

Brussels, Sept. 20,  
1755.

and obedient servant,

T. Brady.

XLI. *A short Account of some new Astronomical and Physical Observations made in Asia; and communicated to Matthew Maty, M. D. F. R. S. by his Excellency Mr. Porter, his Majesty's Ambassador at Constantinople, and F. R. S.*

Constantinople, May 17, 1755.

Read Nov. 6, 1755. **I** Send you inclosed the extract of a French letter I received from a person of great abilities, whose history I cannot immediately communicate to you. It was sent me by the consul of Aleppo. The author is perfectly versed in most branches of physics, as well as in the art of healing.

*Extract of a French Letter, transmitted to  
his Excellency James Porter, Esq; by the  
English Consul at Aleppo.*

Sina, Dec. 14, 1754.

I Have determin'd, with the utmost exactness, several latitudes and longitudes. You know, Sir, of what importance it is to have in the east some fixed points, by means of which, as well as itineraries, one might construct more correct maps than those, which have hitherto been published. I have the honour to communicate to you some latitudes of the principal places, which I have observed, and my observation of a star's occultation by the moon.

Aleppo. Lat. North	-	-	-	-	36 <sup>n</sup>	12'
Mount Cassius	-	-	-	-	36	4
Seleucia in Syria	-	-	-	-	36	3
Antioch	-	-	-	-	36	10
Diarbekir	-	-	-	-	37	54
Bagdad	-	-	-	-	33	19 54''

Immersion of  $\omega$ . Virginis under the Moon, observed June 10, 1753,  
at Diarbekir, near the Seraglio of the Bachaw.

1753, June 10. Altitudes of ☉ at 9 <sup>h</sup>	56'	4''	a. m.	30°	A
☉	58	55	-	30	B
☉ 10	4	13	-	28 30'	C
☉	7	4	-	28 30	D
Corresponding Observ. p. m.	A	2	3	32	
	B	2	-	43	
	C	1	55	28	
	D	1	52	35	

The Immersion of the Star was observed June 10, at 9<sup>h</sup> 48' 4''  
at night.

The Emerfion        -        -        -        -        9 39 47

June 11. Altitudes of the Sun. a. m. Corresponding Observ. p. m.

☉ 7 <sup>h</sup> 28' 13"	☉ 58° 30'	} - - -	4 <sup>h</sup> 28' 53"
7 30 54	☉ 58 30		4 26 15
7 35 53	☉ 57		4 21 16
7 38 33	☉ 57		4 18 36

Sina, or Sneirne, in Mr. de Lisle's Maps.

Lat. North. 30° 9'

34° 23' 35" That Village is no farther distant from Hamadan than 8 common Leagues, of 25 to a Degree.

I have begun my observations upon astronomical refractions, which here are somewhat less than in Europe. I think myself sure, that the vast number of stars, which one imagines to see in Europe, in a clear winter-night, is not a mere illusion caused by the scintillations of the stars, marked in the catalogues, and upon Blaew's globes; but that it comes from those very stars, which here we observe at simple sight, and in Europe, only by means of short telescopes, as the air there is much more loaded with vapours than it is here. Yet, whatever travellers may say, I always observed here some scintillations.

That the nitre is produced by a combination of the universal acid with the natrum of the ancients, appears by my observations. The *asa foetida* is drawn from a ferulaceous plant of the *thapsia* kind, which plant is very common in Media, &c. I have had the good luck to find the small *nardus Indica*: It is a gramineous plant, of which some bear spicaceous flowers, both male and female, and others only female ones. I send you some seeds of this plant. It is a valuable thing to botanists, as they are hitherto ignorant of the true genus of this plant, tho' the root is in use ever since the age of Dioscorides.

I have



I have dried about two thousand plants, among which are several new genera, and some species hitherto undescribed.

I sometimes amuse myself with electricity. This country is so dry, that the experiments often succeed without any stand of bitumen, pitch, filk, glass, &c. Our carpets and beavers are mostly sufficient to retain the electrical virtue, and prevent its spreading to the floor. Ten men standing upright, one before the other, have been made electrical, and, being touched, have produced sparks.

XLII. *Some Observations proving, that the Fœtus is in part nourished by the Liquor Amnii.* By Malcolm Fleming, M. D.

Read Nov. 13,  
1755.

**B**ESIDES the mysterious affair of generation, besides the manner of the formation of the embryo, and the changes it undergoes, while it continues extremely small, concerning all which we shall ever remain very much in the dark; there are not a few things relating even to the mature fœtus, which create matter of dispute amongst anatomists and physiologists. Of these the manner of its nourishment *in utero* is not the least important.

The present state of the controversy concerning this matter turns on the following precise point, to wit, whether the fœtus in utero be nourished solely by the blood, which is transmitted to it through the umbilical

umbilical cord ; or whether it is likewise nourished in part by the liquor amnii, in which it swims. I have said the present state of the controversy ; for I believe very few, if any at all, will maintain now-a-days with Claudius de la Courvée and Stalpart Vander-wiel, that the whole of its nourishment is conveyed by the mouth, and none at all by blood-vessels.

Both sides of the question have been espoused by able writers, and supported with great acuteness, erudition and industry. And as there still subsists a division amongst candid and intelligent inquirers concerning this matter, it would appear, that some decisive fact, some demonstrative argument hath hitherto escaped notice, which, if clearly known and duly attended to, would have by this time ended the dispute, and rendered the proposition, which declares the truth, on which-soever of the sides, an object of science ; which before was only matter of opinion.

An observation, which fell in my way two years ago, together with some subsequent ones, which I purposely made in pursuance of the hint suggested by the first, will, I hope, enable all, who candidly consider the facts themselves, and the consequences that naturally flow from them, to determine certainly, on which side the truth lies.

Before I relate my observations, let me lay down a necessary preliminary, which I observe the writers on both sides are either explicitly or implicitly agreed in ; to wit, that if it be clearly made out, that the liquor amnii is naturally received into the mouth, stomach, and intestines of the foetus, swimming in  
it

it ; in that case we are to conclude, that the foetus is in some part nourished by it. The whole tract of the alimentary passage abounds with absorbent vessels in the foetus, more than in the adult animal ; and especially the small intestines have lacteals plentifully opening into them. The liquor amnii is concreted by heat, like the white of an egg ; which characteristic in animal juices is, I believe, denied by none to be a proof of their alimentary nature. To such as will not grant this postulatam, if any such there be, this paper is not addressed. *Contra negantem principia non est disputandum.*

But I proceed to relate the observations.

On the 25th of July 1753, being informed, that a calf, come to full maturity was just then brought forth dead in this town (Brigg, in Lincolnshire), which had been alive, and appeared strong a very short time before its birth. I begged it of the owner, such instances being rare. The skin being of value, for it was an extraordinary large calf, it was sent to my house stay'd. I first examined the thorax, which was my chief motive for begging it. All I shall mention now of what I observed there, is the experiment of the lungs of a new-born animal their sinking in water. After cutting out the lungs and heart, I clipped off a piece of the former with sharp scissors, about an ounce weight, or more, and threw it into a basin full of water. It quickly sunk to the bottom, and settled there. Immediately after, I blew into the remaining part of the lungs, through the trachea ; and though I could by that means distend them but very little, because the air flowed out readily through the cut bronchia, and therefore acted but faintly

faintly on the other parts; yet a piece about the same bigness with the first, clipped off in the same manner, and thrown into the same basin, constantly kept at the top. This may seem foreign to my present purpose; but I thought proper briefly to mention it here, not only upon the account of the importance of the experiment, but likewise to shew, that I was not misinformed in the account of the calf's being brought forth dead, and that it had not even respired; much less taken any nourishment after exclusion, to influence the appearances, which we are now going to describe.

Having done with the thorax, I opened the abdomen, in order to take a slight view of it before the lumbering carcase should be removed. I observed the thick intestines, especially the rectum, extremely distended with an incredible quantity of meconium; which for several inches above the anus was formed into distinct scybala or balls. It came into my head to make an incision in the rectum, where it was very turgid, about two inches from the anus, and to let out about twenty-five or thirty of these scybala; which I laid upon clean paper to dry, that I might examine them at my leisure. About three or four days after, when they were dry and brittle, of the colour and consistence of aloes; I was surprised to find, upon examination, every ball stuck full of tough, thick, white hairs, some of which were an inch long, or more. There seemed to be some scores in each, though they, being shrunk with drying, did scarce exceed the bulk of an ordinary pea. This unexpected appearance set me a considering, whence these hairs had come; how got they there? And I

could think on no other tolerable solution of the difficulty than to conclude, that they belonged originally to the calf's skin; and, being loosened by maceration in the liquor amnii, were propelled into the stomach and intestines; till they were at length entangled in the meconium. I was confirmed in the belief of this by being informed, upon inquiry, that the calf's skin was white; a circumstance unknown to me before, it having been sent flay'd, as hath been already said.

From this persuasion it was natural to infer, that if hairs loosened from the skin of the fœtus, and floating in the liquor amnii, can find a way into the intestines, and get entangled in the meconium, it is impossible but the liquor amnii must enter and pass through the whole alimentary passage along with them; as a fluid may certainly penetrate where hairs cannot: but no good reason can be assigned, or even conceived, why hairs should be admitted where the fluid is excluded.

The only reasonable scruple, that remained to be got over, was, that this being but a single instance, a general conclusion was not to be too hastily drawn from it; that it was possible there might be some morbid concretions in the meconium of this particular calf, resembling hairs, which concretions in a common and natural way might be wanting; or some preternatural communication between the primæ viæ in this subject, and the liquor amnii, not to be found in the generality of other fœtus's.

To obviate all this, and farther to establish and illustrate the truth of my conclusion, as instances like that, which we have now related, cannot be commanded,

manded, I laid myself out to procure the first dung of calves after they are brought forth ; which always can be nothing but meconium. This was attended with some difficulty and delay, as cows in this neighbourhood mostly calve in the fields. After some months, by the good offices of a sensible acquaintance in this place, I obtained what I wanted. He was so obliging as to watch his own cow's calving, and before he stirred from her, to catch the calf's first dung, and send it me forthwith. Some weeks after another parcel of the same kind was sent me from another quarter. In both which I found a great number of strong hairs all over ; so as to leave no room for doubting but that this appearance is general in the meconium of calves, in a natural way.

The reader will please to observe, that in neither of these instances I could be deceived, if I had ever so little reason to trust to the judgment and fidelity of those, who supplied me with what I wanted. The colour and consistence of the meconium of a foetus is so very peculiar, and so widely different from that of fæces formed out of ingested aliments, that none, who have any knowlege in these matters, can mistake the one for the other.

In the mean time I omitted not to open the embryo's of the cow-kind, such as I could procure in the shambles of the market-town I live in, and to examine their meconium. The two most advanced towards maturity, which I met with, had stiff long hairs about the mouth, the eye-brows, the ears, and navel, and a good many on the end of the tail ; but none on their skins. In neither of these, any more than in the younger embryo's, which I examined,

was there so much as a single hair to be found in the meconium; for this plain reason, if I judge right, to wit, because they had not got hairs upon their bodies of long enough continuance to become loose, and float in the liquor amnii.

But as opportunities of coming at fœtus's of this species, especially such as are remarkably nearer to maturity than those two I have just now mentioned, are rare with us, I tried to supply that defect by opening those of other animals. Accordingly I procured six puppies, of the butcher-dog-kind, brought forth at the full time, at one litter. Having taken out the whole meconium of every one of them, after the strictest search, I could find no hairs in any part of it. I had likewise an opportunity of opening a colt, that died either in the birth, at the full time, or immediately after, before its meconium was discharged; which I found in great quantities in its rectum and colon. But neither here could I spy a single hair, though I examined whole pounds of it, and that portion thereof most carefully, which was lodged in the rectum, near the anus.

These observations may seem at first view to clash with and contradict those I have related: But, upon closer consideration, they will be found in reality to confirm them, for this reason, to wit, that puppies and colts, when brought forth, have no loose hairs on their bodies; but calves have in great numbers. In the puppies and colt, which I examined, the hairs were so firmly rooted on their skins, that I could scarce pull any off with my thumb and fingers; whereas in a ripe calf, new brought forth, many are found quite loosened at their roots, and only adhering

ing to the skin by the moisture on it. Therefore in species, hairs from the surface may be, and really are, incorporated with the liquor amnii, and going with it enter the mouth and alimentary canal, which cannot be the case in the former.

From these facts it is easy to draw such inferences, as are sufficient to decide the controversy, which we are treating of; and prove incontestably, that the liquor amnii is in a constant natural way received into the mouth, stomach, and intestines, and therefore must contribute to the nutrition of the fœtus.

For if we attentively consider, that in the meconium of an human fœtus no hairs are found, there being no loose hairs on its skin when it is born; that in the meconium of unripe calves there are none, before they are of so long continuance on their bodies as to be loosened, and float in the liquor amnii; whereas in that of the ripe calves they are found in great abundance, of the same colour with those of the skin, as appears by the example we had occasion to examine most particularly; and of such a length, stiffness, and thickness, and so constantly to be met with, as cannot with any shew of reason be imputed either to morbid concretion, or preternatural frame: If, I say, all these facts are attentively considered, and duly weighed, the conclusion I have drawn will, I hope, appear to follow from them so naturally, that no candid and intelligent inquirer will call its truth in question.

#### P O S T S C R I P T.

After I had gone thus far upon my own bottom, and while I was looking about me, in order to pick  
up



up more proof, that might, as it were *ex superabundanti*, confirm and illustrate the doctrine, which I was endeavouring to establish, before I should venture to publish it, I casually found what, as I here solemnly declare, I knew nothing of before, to wit, that I was not the first, who had observed hairs in the meconium of ripe calves, two authors having been before-hand with me, to wit, Aldes and Swammerdam; the former in his *Epistola contra Harveium*, published in the first volume of the *Bibliotheca Anatomica* of Mangetus and Le Clerc: the latter in the *Biblia Naturæ*. Aldes, a feigned name under which Slade, an Amsterdam physician, conceals himself, barely mentions the observation, without drawing any inference from it, either on the one side or the other. Swammerdam draws the same conclusion with me; but, I think, he goes farther than the observation can well justify, maintaining, that it evidently follows from thence, that a calf licks his own skin *in utero*, thereby loosening the hairs, and swallowing them down along with the liquor amnii, its nourishment. See the whole passage, *Bibl. Natur.* pag. 319.

However that be, it is enough for my purpose to have two such authors concurring with me, as to the reality of the fundamental fact, and quite saves me the trouble of making any farther trials to confirm it.

It is no wonder, that these passages escaped me, who have spent almost all my time in practice, and practical studies, and who never was possessed of either of these books; but, after I found them quoted, was obliged to trouble two learned friends, at a dis-

stance, upon this occasion, for my own satisfaction, and that very lately. The one was so obliging as to send me the *Bibliotheca Anatomica*; the other to write out at large the passage from the *Biblia Naturæ*. I say, it is no wonder the passages escaped my notice, since they seem to have been overlooked by not only all the particular writers on the foetus, since Aldes and Swammerdam, but likewise, for aught I know, by all the lecturers on physiology in Europe, preceding August 1753; at which time I began to communicate by letters, what I then took to be a discovery, to several correspondents versed in these matters, both at home and abroad, as I can vouch by their answers.

So that I hope the public will do me the justice to believe, that though I was not the first in point of time, that made the fundamental observation, yet I was obliged to no author for it; and that I have drawn an important truth relating to the animal oeconomy out of obscurity, and established it by supplemental observations, as well as by reasoning.

Before I conclude, let me acquaint the reader, that an ingenious student of anatomy and surgery \*, to whom I had communicated my first observations, and recommended to him to examine the liquor contained in the stomachs of ripe new brought forth calves, if he had a proper opportunity, acquainted me last harvest, that he had not long before actually found hairs in abundance in the liquor contained in

\* Mr. John Clark, now surgeon to the Lock-Hospital, near Hyde-Park Corner.

one of the stomachs of a calf, brought forth dead at the full time, of the same colour with those on its skin. As this observation renders the proof, however clear and unexceptionable it was before, fuller, I was unwilling to omit it here.

Brigg, July 12,  
1755.

Malcolm Flemyng.

**XLIII.** *An Account of the Success of Agaric in Amputations, &c. in a Letter from Mr. William Thornhill, late Surgeon to the Infirmary at Bristol, to Robert Dingley, Esq; F. R. S.*

Novem. 2, 1755.

Read Nov. 13, 1755. **I** Have made use of the agaric in four amputations: the first was on the arm, between the elbow and wrist; the second and third were below the knee; and the fourth on the crural artery. In every case it answered beyond my expectation; the patients had little or no fever, and no spasms at all, which are frequently the consequences of the ligature, and sometimes fatal. I am surprised, that any objections are made to the use of it. I fear the true reason of its not being used in general practice proceeds from a narrow selfish way of thinking; but it is my opinion it will make its way, to the honour of its first author. I am well satisfied, it is the best medicine in all the materia medica yet discovered for that purpose.

I have

I have tried it in female cases, with great success, by injecting a strong decoction of it in hæmorrhages from the uterus, and particularly in fluxes of that kind after delivery.

XLIV. *Lunæ defectus Elbæ à Doctore Joanne Mendefio Sachetto Barbosa, Philosophiæ & Medicinæ Professore, Regiæ Societatis Londinensis Socio, & Medicæ Academiæ Matritensis, Regalis Elbensis Nosocomii Medico, observatus Die 27--28 Martii, Anno 1755. Communicated by J. De Castro Sarmento, M. D. F. R. S.*

Read Nov. 20, 1755. **N**ON telescopio, sed tubo optico satis claro & longo, hoc lunare deliquium observavi: eclipseos tamen initium, vel instrumentorum debilitate, vel usus defectu, accurate determinare vix potui. Ne tamen apparentias pro veritatibus acciperem, initium non nisi quando de eo nemo claræ mentis & visus dubitare poterat annotavi. Idem de fine dicendum; veruntamen tempus a me datum pro proxime certo haberi potest. Pro certo, inquam, non respectivo ad tempus verum, quia quadrante, & meridiana fixa certa destituti sumus; sed ad tempus à solari nostro horologio horizontali indigitatum. Fatemur tamen hoc horologium solare, secundum regulas gnomonicas, aut non exactè descriptum, aut non perfectè locatum nobis videri.

Aer ferenus erat, & calidus ad usque 54 gr. thermometri Farenh. sed his non obstantibus, tali modo lunares faculae sub umbra occultabantur, ut de illis nihil omnino translucebat. Per immersiones datas, maximam obscuracionem notare tantummodo potui. Sic umbra, prima Aristarchi & Maris Crisium limina quasi tetigit, & veluti lambit; attamen iis leviter tactis sistit, & gradus revertitur. Ac ideo eclipses quantitatem ad justum, assistentium & instrumentorum penuria, minimè signare audeo; ex dictis tamen, & ex mensura quadam arbitraria ad digitos 8 & 12' pervenisse videtur. Media Marium nonnisi æstimatorè notantur.

## IMMERSIONES.

h. ' "			
10	51	15	Eclipseos initium jam certum.
10	56	37	Intra Mare Humororum & Grimaldum.
10	59	17	Grimaldus totus, & Mare Humororum per medium.
11	4	27	Intra Mare Nubium.
11	10	8	Absorbetur Galilæus.
11	11	39	Ad medium Maris Nubium.
11	15	17	Totum Mare Nubium.
11	20	37	Reinholdum lambit.
11	30	27	Umbra intra Copernicum.
11	32	16	Intra umbram sinus Medii.
11	33	57	Totus Copernicus.
11	37	33	Intra Mare Nectaris.
11	39	7	Per medium Maris Nectaris.
11	44	8	Totum Mare Nectaris.

h.	'	"	
11	47	8	Tangit Mare Tranquillitatis, Mare Vaporum *, Mare Imbrium, & Eratosthenem.
11	50	8	Intra Mare Fœcunditatis.
11	56	8	Totus Eratosthenes.
11	57	43	Pitheam absorbit umbra †.
12	00	8	Medium Mare Fœcunditatis.
12	1	38	Tangit Manilium.
12	4	8	Totum Mare Fœcunditatis.
12	13	58	Menelaus, & medium Mare Tranquillitatis.
12	16	25	Tangit Plinium.

#### EMERSIONES.

12	12	8	Emergit totus Marfilius.
12	20	8	Eratosthenes.
12	22	8	Galilæus.
12	23	14	Incipit emergere Copernicus.
12	24	18	Copernicus totus.
12	27	23	Totus Grimaldus.
12	28	28	Medium Mare Tranquillitatis extra umbram.
12	23	43	Incipit Reinholdus.
12	35	21	Totum Mare Vaporum ‡.
12	44	8	Emergit Dionysius.
12	47	28	Incipit Mare Fœcunditatis.
12	48	53	Apparet Promontorium acutum.

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\* Hoc Mare notamus secundum descriptionem Caille, in Lect. Astronom. pag. 118. quam hac in parte verisimiliorem inveni.

† Secundum eundem Autorem.

‡ Vide notam asterismo indicatam.

h.	'	"	
12	52	45	Medium Mare Nubium.
12	55	28	Totus Schicardus videtur.
1	00	58	Medium Mare Fœcunditatis.
1	1	26	Totum Mare Nubium & Tranquillitatis.
1	4	15	Incipit emergere Mare Nectaris.
1	7	40	Totus Tycho.
1	12	40	Totum Mare Nectaris, & Fœcunditatis.
1	27	40	Finis totalis Eclipseos.
1	31	30	Extinguitur penumbra de Limbo Lunæ, nemine dubitante.

XLV. *A Letter to George Lewis Scot, Esq;  
F. R. S. concerning the Number of People  
in England; from the Reverend William  
Brakenridge, D. D. Rector of St. Michael  
Bassishaw, London, and F. R. S.*

Dear Sir,

Read Nov. 20, 1755. **H**AVING lately wrote to you my ob-  
servations, concerning the number  
of inhabitants in London and Westminster, this has  
led me also to consider, whether there may not be  
some way to compute nearly, the number of people  
throughout England; and if it can be done, there  
are many political uses that may be made of it, and  
many conclusions that may be drawn, which may  
prevent mistakes in Government, and that will tend  
to promote the strength and riches of the Nation.  
The subject is indeed intricate, and there cannot be  
that

that accuracy in such calculations as might be desired; but I imagine we may come so near the truth, as is sufficient for any purpose to reason upon, or to be a foundation for any speculations in Policy. As you are a good judge of such computations, I presume to send you what I have done; and if I have your approbation, I shall be more satisfied that I am in the right; but, if I be mistaken in any particulars you will, I hope, make great allowance for the failure, when you consider the difficulties that occur.

There seems to me to be only two ways of discovering the number of people in England, where at present there are no Capitation Taxes; either by the number of *Houses*, or the quantity of *Bread* consumed. I shall consider both these methods of computing, so far as things are known to me; and the result from each of them being compared together, they will correct one another, from which at least the limits of the number may be nearly found. As to the first it is evident, that if the number of houses could be determined, it would then be very easy to compute nearly the number of people. For it might be easily known by trial, what number, at an average, could be allowed to each house, and from thence the whole number of people deduced. In my letter last year I have assigned six to a house in Town, which I found to be the nearest number, in some Parishes, by an account taken; but I think it is still more plain in the Country, that six is the number to be fixed on, where people do not go so much into single life, and where there are not so many Lodgers. For if we consider, that for every marriage there are  
four



four births, at an average, as Dr. Derham, Major Graunt, and others have shewn, and which I have found to be true from the Registers both in the Town and Country ; then consequently, allowing for deaths, there cannot be three children that survive, from every marriage to mature age, and indeed not much above two, as appears from Dr. Halley's Table of the probability of life. And therefore every family, where there are children, one with another, cannot consist of more than between four and five persons, besides servants or inmates ; which shews plainly that families, where there are children, cannot be estimated at more than six to a house, and where there are no children, they cannot be reckoned more at an average.

The number then being six to be assumed, let us next consider what number of houses is to be supposed. That I might come at some certainty in this I lately applied to one of the Public Offices, where I thought they could very likely give me an account of them ; and I there found, that before the year 1710, and near about that time, an account had been taken of all the houses throughout England and Wales, in order for some Assessment upon them ; and the number then did amount to 729048. In which it may be supposed, that a number of Cottages were omitted, that might be improper for that Assessment ; but I think there could not possibly be above one-fourth part of that number more : For surely the Surveyors, if they had any care of the Public Revenue, would never omit above one in Five. Let us therefore suppose, that there might be one-fourth part of that number more ; and then those omitted will be about 182262, and the whole number of houses could not exceed 911310.

If

If now we take 911310 for the number, it is evident, if we allow fix persons to a house at an average, according to what has been mentioned, the number of persons through England and Wales, before the year 1710, could not be above 5467860. And since that time, 45 years ago, by a method of computing which I shall presently shew, the increase could not be above 789558; and so the whole number of people now must be about 6257418; or fix Millions, all ages included; for it must be remembered, that in our wars since 1710, there could not be fewer lost than 200000, which is to be deducted from that number.

As to the other way of determining this, by considering the quantity of *Bread* that is consumed, it may perhaps at first view appear more uncertain; but it will, I think, from some things that may be observed, at least help to ascertain the above number. For it is plain, if the quantity of *Wheat* that is produced in England could be known, it would then be very easy to make the computation, as it might be nearly discovered, by a little observation, what each person at an average might consume. But the great difficulty is to find out nearly the quantity of *Wheat*; and there seems to be no way at present of knowing it, but by considering what proportion it may have to the *Barley*; for the quantity of that is nearly known from the Malt-tax.

Now, if we compare the quantity of the *Barley* with that of the *Wheat* in England, it is evident, that there is at least as much ground sowed with the one as with the other. For there are vast tracts of land that will not bear good wheat, but are frequently sowed  
with

with barley; and even those lands that will produce good wheat, they are often alternately sowed with it: The land that is rich and well manured, after one crop of wheat, it is usual to sow it with barley. And if this be admitted, that the quantity of land sowed with the one is equal to that sowed with the other, there must then be a much greater quantity of barley; because the same number of acres will produce much more of it, and generally in a greater proportion than Three to Two. These facts are so well known that I believe every reasonable Farmer, when he considers them together, will allow, that the barley cannot be in a less proportion to the wheat than Three to Two.

If then we allow, to make a calculation, that they are in this proportion, though I think, from what I have observed in the Country, the difference is greater, we shall then nearly find the quantity of wheat from the Malt-tax; because the general consumption of barley is in malt. To state this as exactly as we can, it must be remembered, that there are about 500000 quarters of wheat almost annually exported, as appears from the Bounty-money paid by the Government; and some of it is made use of in mechanical purposes at home, besides food. And on the other hand there is some barley exported, though nothing near to the quantity of wheat, and a great deal of it given to fatten Hogs; so that the barley exported, and what is given for the purpose of fattening, may be thought to be equal, or more than the wheat exported, together with what is used for mechanical purposes. Let us imagine that they are equal, for the error will not be great in this gross manner

manner of computing, and then the remaining quantity used in malt must be in a greater proportion to the remainder of the wheat used in food than Three to Two.

If then we assume this, that the barley used in malt is to the wheat used in food at home, as Three to Two, though I believe the malt is in a greater proportion to the wheat, we shall then be able to compute the quantity of each of them in this manner: The Malt-tax from the year 1747 to the year 1753 inclusive, amounted to the sum of 4,254813*l.* of which the seventh part, the Tax for one year, is 607830*l.* and as the Tax is four shillings upon every quarter of barley, it follows, that there are 3039150 quarters of barley consumed yearly in malt; and therefore, by what has been said above, that this quantity cannot be in a less proportion to the wheat than Three to Two; there must be 2026100 quarters of wheat consumed at home. Now, as it is known, and I have observed it in the country, that labouring healthy people, at an average, consume about one quarter of wheat in the year, which is about 512 pounds of flower, or one pound and six ounces in a day, we may allow that healthy and unhealthy, grown people and children, do not consume the half of that quantity, one with another. And therefore, that we may make the consumption of each person at an average, as small as can reasonably be imagined, we will suppose that three people, children included, do not consume more than one hearty labouring person, that is one quarter in the year, or each person about seven ounces in a day; and by this supposition, the above number of quarters of wheat 2026100, consumed at home,

will be sufficient for 6078300, or six millions of people. And this quantity of a quarter to three persons, though it appears too little, may be admitted, as in some of the northern counties they use some Oat-bread and Rye-bread; and every healthy person may, one with another, be allowed to consume this quantity at least.

From this calculation it seems to me to be evident, that there cannot be above six millions of people in England: For the barley is certainly not in a less proportion to the wheat than Three to Two. And the quantity of wheat exported, and used in mechanical purposes at home, is not much less than the barley used in fattening, together with what is exported. And therefore the quantity of wheat used in food at home cannot exceed 2026100 quarters, which, computed at any reasonable rate, will not be sufficient for more than six millions of people; because it must always be supposed that labouring persons, or those in lower life, who have no great variety of food, consume much more bread, or food of wheat, than others in a more wealthy condition; and seven ounces a day, at an average, is rather too little allowance. And as from the other method of computing from houses, we found the number to be about 6,257400, from which at least 200,000 is to be taken for those lost in our wars since 1710, or near that time; it appears that both these calculations confirm one another, and that the number of people may be considered at about six millions, or rather less. In which, according to Dr. Halley's rule, there will be about fifteen hundred thousand men able to carry arms.

The worthy Dr. Derham, from the computations of Mr. King, which I never saw, supposes there is about five millions and an half of people in England ; to which, if we add the increase that may be since that time, the number will be near about what we have made them. But Sir William Petty has endeavoured to make them, in his time, no less than 7369000, by supposing them to be in proportion to the Assessment, then eleven times greater than that in the city of London. In which, with regard to the city, he was certainly mistaken, as I have shewn last year ; for the number at that time, in 1682, was not much above 504000, and therefore eleven times that, viz. 5544000 must, according to his own hypothesis, be the number of people in England. And if we allow 1355000 to be the increase in about 73 years since that time, by the method I shall presently shew, the number could not be now, according to that assessment, above 6899000. From which we ought at least to subtract 400000, which may be justly allowed for loss in our wars since 1690 ; and the remainder 6,499000 is not half a million more than we have made them. But to compute the number of people from any pecuniary assessment that must arise from trade, circumstances, and valuation of land, seems to me to be a much more uncertain method than either of these I have used.

The people then being computed at six millions, or rather less, it appears that England is but thinly peopled. For not only the exportation of at least 400,000 quarters of wheat annually shews plainly, that we want people to consume it at home, and that we maintain in bread about a million of foreigners abroad :

but if we examine more particularly we shall find, that the Country is capable of supporting one-half more inhabitants, or nine millions. According to Mr. Templeman's survey, England contains 49450 square miles, that is 31,648,000 acres, because a square mile is equal to 640 acres. And if we suppose one-fifth of it waste ground, heaths, &c. there will remain about 25,300,000 acres of land proper to be cultivated. And as it can easily be made appear, that three acres, well manured, is sufficient for the maintenance of one person, I mean if a great number of acres are taken together, to produce the various necessaries of life in victuals and cloathing, then there will be maintenance in England for 8,430,000 people, children included; which, with the advantage of fishing, that the situation of the country gives, we may well allow that there is sufficient provision for nine millions of people, that is three millions more than we have at present. And this is only from the natural produce, without any of the advantages from trade, and the help of our Colonies in America, by which double the number might be maintained. The above allowance of three acres to each person, I think is too much; but some consideration must be had of the inclosures and pleasure-grounds, which those in higher life will always have.

But in Ireland the case is still worse: For if there is but a million of people, as is commonly supposed, and according to Mr. Templeman 27400 square miles, which is 17,536,000 acres, and one-fourth or more be supposed waste; then there will be at least 12,000,000 good acres. And consequently if four acres in that country be allowed sufficient, at an average,

average, for the maintenance of one person, Ireland, if duly cultivated, could maintain two millions more people than it has now, or three times its present number of inhabitants.

And in Scotland, if there be, as is said, but a million and an half of people, for at present I know no way to compute them, and 27700 square miles, or 17,728,000 acres, and one-third be supposed waste, which is not too much in that Country, then there will be 11,000,000 good acres; of which, if we suppose that five acres of that soil is not more than sufficient for each person, then there may be provision for 2,200,000 people, or more, with the advantages of fishing, that is 700000 more than there are at present. From all which it is plain, that if the land in both the British isles was duly cultivated, they might sustain about six millions more people than they do now; that is as many more people as England now contains. And this proportion of the number of people, to the quantity of provisions that may be raised for them, from the natural produce of the country, I think is of great importance to consider, as it has many useful consequences, some of which might be mentioned, if this was a proper place for political reflections.

And here, by the way, it may be observed, if we extend our thoughts to the whole Globe of the Earth, and compare the quantity of land with the number of people, we shall find, that it will maintain above twenty-six times the present number of mankind. As this has hitherto not been taken notice of, I shall briefly shew it. The circumference of the Earth, supposing it to be a sphere, is, according to the mea-  
sures.



fures of Messieurs Picard and Cassini, 123,249,600 Paris feet, or 131,630,572 English feet, which is 24930 English miles; and the diameter is 7935 English miles. And then the whole surface of the globe, by Prop. 38. lib. 1. *Archim. de Sphæra & Cylindro*, is  $24930 \times 7935 = 197,819,550$  square miles. And as the whole surface is to the quantity of land, near about 8 to 3, the land will then be 74,182,331 square miles; of which, if we allow one-third to be waste ground, or unfit to produce the necessaries of life, we shall have 49,454,887 square miles, or  $49,454,887 \times 640 = 31,651,127,680$  good acres. Now the number of mankind over the whole globe is computed by Sir W. Petty, and others, to be under 350,000,000; but we will suppose them 400,000,000, which is surely more than their number, that we may avoid any uncertainty in computation; and then there will be 79 good acres to each person. From which it is evident, if the soil in England be considered as a medium between the poor lands in the northern Climates, and the very fertile in the southern, and three acres be here sufficient for one person, that the earth can maintain more than twenty-six times its present number of inhabitants. And if we imagine the land to be in a greater proportion to the surface than 3 to 8, and the number of mankind less than we have supposed, the produce of the earth will then be in a greater proportion to them. And hence it plainly appears, that the earth is in a very imperfect state with regard to the number of people. And that if births and burials are supposed nearly in the same proportion, all over the Globe as in England, it will be above 1000 years before the earth can be fully peopled.

peopled. From which it seems probable, that the origin of mankind is not more antient than is commonly believed.

But now, to return to our purpose, let us in the next place see what may be the annual increase of the people in England, that we may be able to judge of the future improvement of the Country. Dr. Derham, in his *Physico-Theology*, has shewn, from some observations communicated to him, that the number of births are annually to the burials through England in general as 1,12 to 1, though I should imagine the births, if there was an exact account taken, would be in a little greater proportion. From which, if we could know the number of the burials, the increase would be easily found. And in my Letter last year I have there made it appear, that within the bills of mortality there die about one in thirty, and in some very healthy places in the country about one in fifty, which seem to me to be the two extreme degrees of health in England; so that in many Towns, and in fenny or marshy lands, the degrees of health must be between these two. Let us now take the mean between them, which is one in forty, and this will be nearly, at an average, the degree of health through England, or perhaps Britain in general, as Sir William Petty has observed, and which will very well serve to make a calculation of the increase of the people. For then, if we suppose the whole number to be 6000000, the fortieth part of it, viz. 150000 will be the number of the dead yearly. And the births, from the proportion of 1 to 1,12, will be 168000; from which, if we take the number of dead, the remainder 18000 must be the annual increase;

crease; which indeed is very small, and I believe much diminished, by the emigration of great numbers to our colonies in America, and settlements elsewhere, or by our wars, and losses at sea. So that if it was not for the accession of Foreigners, and those who come from Scotland and Ireland, the increase would be very inconsiderable, if any at all; which by the way shews the reasonableness and good policy of encouraging Foreigners to settle among us. However, let us suppose the annual increase to be 18000, and it will be easy from thence to find in what time the number of the people may be double, or in any given proportion; not by dividing 6,000,000 by 18000, considering the annual increase as a constant quantity, which is the method Sir William Petty uses by mistake, or perhaps not knowing how to do it otherwise; but by making this annual increase continually to increase as the whole number does. Let us propose the question first in general, the number of people being unknown, which is this:

The proportion being given of the living to the dead in one year, and also the proportion of the births to the dead, the number of the people being unknown; to find in what time the people shall be in any given proportion, to what they are at present.

Suppose  $n$  to be the unknown number of the people at present; and let the living be to the dead, in one year, as  $l$  to 1, and the dead to the births as 1 to  $b$ , the proportion given to what their number is at present as  $p$  to 1, and the number of years required to be  $y$ .

It is plain then, that the dead at the end of the first year will be  $\frac{n}{l}$ , and the births  $\frac{bn}{l}$ , and the whole number of people must be  $n + \frac{bn}{l} - \frac{n}{l}$ . In like manner, at the end of the second year, the dead will be  $\frac{ln + bn - n}{l^2}$ , and the births  $\frac{lbn + bbn - nb}{l^2}$ , and the whole number of people must be  $n + \frac{bn}{l} - \frac{n}{l} + \frac{lbn + bbn - nb}{l^2} + \frac{n - ln - bn}{l^2} = n \times \frac{l + b - 1 \times l + b - 1}{l^2}$ . And so at the end of the third year the number of people will be  $n \times \frac{l + b - 1}{l^3}$ .

From which at length it is evident by induction, that the number of people at the end of the required number of years will be  $\frac{n \times l + b - 1}{l^y}$ . But as the proportion is then to be as  $p$  to 1, we shall have  $\frac{n \times l + b - 1}{l^y} = p n$ , and from thence  $l + b - 1 = p l^y$ ,

And because the logarithms of equal quantities must be equal, we shall have  $y \times \log. l + b - 1 = \log. p + y \times \log. l$ , and also  $y = \frac{\log. p}{\log. l + b - 1 - \log. l}$ .

And therefore the number of years  $y$  is determined by the logarithms of known quantities when the people shall be in the given proportion of  $p$  to 1.

It may be observed that the quantity  $\frac{n \times l + b - 1}{l^y}$  may be considered as the ordinate of the logarithmic curve, whose abscisse is the index  $y$ , and that the

ordinate passing through the beginning of the abscissæ, where  $y = 0$ , must be equal to  $n$ .

If now it be required to know when the people shall be doubled; let us substitute in the above formula, in place of  $b$ ,  $l$ ,  $p$ , the respective numbers 1, 12, 40, 2, and it will be  $y =$

$\frac{\log. 2}{\log. 40 + 1, 12 - 1 - \log. 40}$ ; and then the logarithms

being taken we shall have  $y = \frac{0,3010300}{0,0013009} = 231$ ;

which shews, that, according to the present state of births and burials, the people could not be doubled in less than 231 years.

And by the same method it appears, changing the signs of  $b - 1$ , that 230 years ago, in the time of Henry the VIIIth, the number could not be above one-half of what it is now, that is about 3,000,000.

And so if we were to find, when the number of people in England would be increased to nine millions, which, by what has been said above, is near about the outmost that can be maintained, from the natural produce of the country; we should then have  $p = \frac{2}{1} = 1, 5$ , because nine millions is to the present number, as 3 to 2, and also  $y =$

$\frac{\log. 1, 5}{\log. 40 + 1, 12 - 1 - \log. 40} = \frac{0,1760913}{0,0013009} = 135$ ;

which shews, that at the present rate of births and burials, it must be 135 years before England can be fully peopled.

If we suppose, as Sir William Petty does, that the burials are to the births as 9 to 10, that is 1 to 1, 111, which is something less than that of Dr. Derham's proportion, and that one dies in 40 in a year; if we substitute

substitute these numbers in the formula, we shall then find the time of doubling to be 250 years. For

$$\text{then it will be } y = \frac{\log. 2}{\log. 40 + 1,111 - 1 - \log. 40} =$$

$\frac{0,3010300}{0,0012035} = 250$ ; which shews how far Sir William was mistaken, in his method of calculation, when he made the time to be 360 years.

After the same manner, the number of years being given, it will be easy to find the proportional increase. Suppose after 45 years. For then we should have  $45 \times \log. \overline{l + b - 1} = 45 \times \log. l = \log. p$  which will give  $45 \times 0,0013009 = \log. p$ . and therefore  $p = 1,1443$ , from which if  $n$  be equal to 5,467,860, we have  $pn = 6,256,872$ . So that it appears if there was 5,467,860 of people in England at the year 1710, when the above-mentioned survey was made, there is now 6250000; if there was none to be deducted upon the account of our wars, and emigrations to our Colonies since that time.

From what has been found above, that  $\overline{l + b - 1}^y = p^y$ , it is evident, that the ratio of the increase in any number of years may be determined, without the number of people being known, or their proportion to the annual increase; and also that any one of the quantities  $l, b, y, p$ , may be found, the others being known. But if the ratio of the number of people to the annual increase be known; and consequently the proportion, of the number in any one year, to the number next year known, we shall then have a very simple equation. For if we suppose the number of people in any one year, to be

cession of Doctor William Oliver, physician at Bath ; who has placed them in his garden, and been so obliging as to transmit to me draughts of them, with their inscriptions, taken by the Reverend Mr. Borlase, a worthy member of this Society, which accompany this paper (1). And since that I received from Mr. Prince Hoare casts of the inscriptions in plaister of Paris, by which means I have had the advantage of comparing them together.

1. The inscription upon the higher altar may, as I apprehend, be thus read in words at length :

*Peregrinus Secundi filius, civis Trever, Jovi Cætio, Marti, et Nemetona, votum solvit libens merito.*

The person, who dedicated this altar, calls himself PEREGRINVS SECVNDI FILIVS; each of which names occurs several times in Gruter, as a cognomen, which often stands alone, when the person named is sufficiently distinguished by it.

Having given us his own name, and that of his father, he proceeds to acquaint us with his country, and styles himself CIVIS TREVER. So in Gruter we read CIVIS TREVERA, in the feminine (2); but Tacitus has *Trevir*, with an *i* in the latter syllable (3). Tho when he speaks of these people collectively as a nation, in the plural number, he always calls them *Treviri*, with an *e* in the middle syllable, in which he is followed by later historians; but in Caesar they are always written *Treviri*, with an *i*. They inha-

(1) See TAB. VIII. Fig. 1, 2.

(2) Page xiii. num. 5.

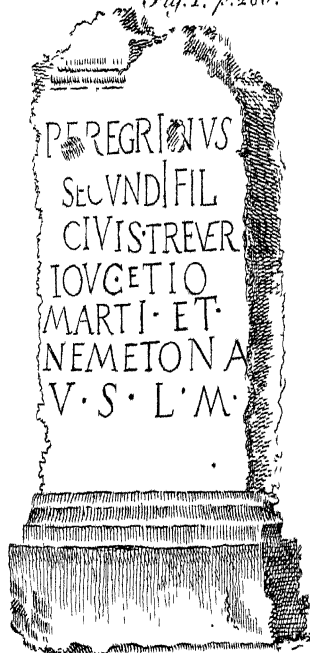
(3) *Hist. Lib.* iii. cap. 35.



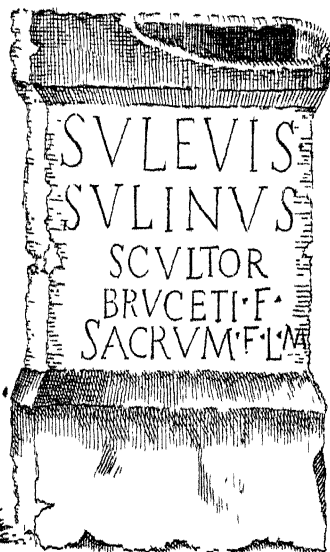




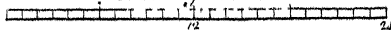
*Fig. 1. p. 286.*



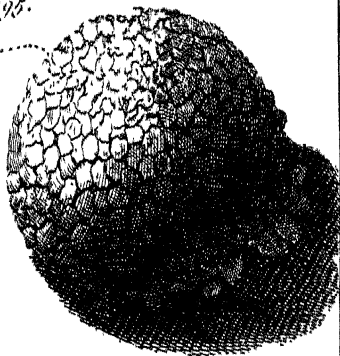
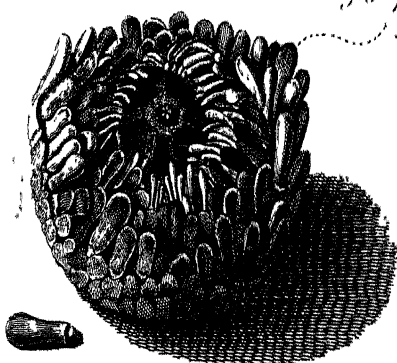
*Fig. 2. p. 292.*



*A Scale of Inches*



*Fig. 3. p. 295.*





bited that part of Belgic Gaul between the Maese and the Rhine, which is now the electorate of *Triers*; and were conquered by Caesar, with the rest of the Gallic nations. But Pliny speaking of them says, they were *liberi antea* (1), that is, tho subject to the Romans, yet, with some limitations, permitted to govern themselves by their own laws (2). By the words *liberi antea* Harduin understands him to mean, that they were free both before that time, and then continued so; but others think his design was to intimate, that tho they had before been free, they were then deprived of their liberty by Vespasian, on account of their joining with Civilis in the late disturbances of the Roman government (3). But I find no particular mention of this in antient writers; and it is plain from Vopiscus, that they enjoyed this privilege long afterwards. For in his life of the emperor Florianus he recites a letter, written by the Roman senate to that of the *Treveri*, in which is this expression: *Ut estis liberi, et semper fuistis, laetari vos credimus* (4). Their cheif city, which was situated on the Moselle, being made a Roman colony in the reign of Augustus, is by Tacitus called *Colonia Treverorum*, but by others more frequently *Augusta Treverorum*, and now *Triers* (5).

The three following lines of the inscription contain the names of three deities, to whom this altar

(1) *H. N. Tom. I. pag. 224. l. 5.*

(2) *Strab. L. xvii. pag. 839.*

(3) *Cellar. Geograph. antiq. Tom. I. p. 201.*

(4) *Cap. 5.*

(5) *Cellar. ibid. p. 202.*

was dedicated. The first of these is here called *JUPITER CETIVS*. Ptolemy makes mention of a large mountain in Germany, which he calls Κέτιος, and describes as the eastern boundary of *Noricum* (1), by which it was separated from *Pannonia*, now *Hungary*. From this mountain it seems highly probable, that the name *Cetius* might be given to *Jupiter*, as its tutelar deity. So likewise he was stiled *Casius* from a mountain of that name in Aegypt, where, as Strabo informs us, a temple was erected to him under the title of Διὸς Κάσιου (2). And indeed there was scarce any mountain, or other place, of considerable note, where some pagan deity, either male or female, was not more particularly worshipped as its protector and guardian, tho at the same time they had their votaries likewise elsewhere. There was also a town called *Cetium*, which was situated at the foot of this mountain, towards the Danube, and not far from *Vienna* (3). It is mentioned both in Antonine's *Itinerary* (4) and *Peutinger's Tables*, tho in the latter it is corruptly written *Citium* (5); for it plainly appears, that the same town was designed in each, by its situation. However it seems more probable, that *Jupiter* had the title *Cetius* ascribed to him from the mountain, to which the town likewise owed its name; tho let either be supposed, it will make no difference, with regard to the explication

(1) *Geograph. Lib. ii. cap. 14.*

(2) *Lib. xvi. p. 760.*

(3) *Cellar. ubi supra, pag. 342.*

(4) *Pag. 234. edit. Wesseling.*

(5) *Segment. ii. in Norico.*

here given of this inscription. In the name *Cetius* indeed, as it is written on the stone, the two first letters c and e are separated from each other by a point. But that doubtless must have been occasioned either thro the ignorance or inadvertency of the workman, in misplacing the point, which should have stood after the imperfect word *iov.* that immediately precedes. Mistakes of this kind are not uncommon, two or three of which were observed in one of the *Wroxeter* inscriptions, some account whereof I had not long since the honour to lay before this Society (1). But it may be further observed concerning this *Jupiter Cetius*, with relation to the place of his worship, that he must have been a German deity; whereas the *Treveri* were inhabitants of Gaul, on the other side of the Rhine, which divided those two countries. However, no impropriety will arise from thence, in supposing him to have been worshiped by them; for most of the Belgic nations, as Caesar informs us, sprang originally from the Germans; but having formerly passed the Rhine they settled themselves in Gaul, on account of the fertility of the country, and drove out the antient inhabitants (2). And particularly with regard to the *Treveri* Tacitus says: *Circa affectationem Germanicae originis ultro ambitiosi sunt, tanquam per hanc gloriam a similitudine et inertia Gallorum separentur* (3). It cannot seem strange therefore, if being thus tenacious of preserving the memory of the country, from whence

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(1) See above, pag. 196.

(2) *B. G. Lib. ii. cap. 4.*

(3) *De moribus German, cap. 28.*

they originally came, they continued to worship the German deities after their settlement in Gaul.

And that they did so, will further appear from the next deity here addressed to, which is MARS; who was not only worshiped by all the Germans, but likewise in the highest esteem among them. For one of their ambassadors in a public speech made to the Belgic Gauls, as given us by Tacitus, thus expresses himself: *Redisse vos in corpus nomenque Germaniae communibus deis, sed (1) praecipuo deorum Marti, grates agimus (2).*

The third and last name here mentioned is NEMETONA, which I have no where else met with; but as it stands connected with the two former by the particle ET, it must, I think, denote some deity, and by the termination a goddess. For thus we have *Bellona, Hippona, Latona, Pomona*, with other names of female deities, in the like form. The construction of the sentence seems indeed to require, that it should have ended with the diphthong AE, instead of the vowel A, as it does in the inscription; tho as that letter stands at the very edge of the stone, the workman might find himself obliged to leave it so. But as nothing further appears concerning this goddess *Nemetona*, it seems most reasonable to suppose her to have been one of those topical deities, several of whose names are found once only in Roman inscriptions, but never mentioned by their writers; some of which occur among our British inscriptions, as

(1) The Florentine MS. has *et* in this place instead of *sed*, and so it was read by Sir Henry Savile.

(2) *Hyt. L.b. iv. cap. 64.*

*Brigantia, Cocis, Matunus, Setlocenia*, and others, which may be seen in Horfley (1).

The last line of the inscription acquaints us with the cause of erecting this altar, which was the performance of some vow, formerly made by *Peregrinus*. And it is not improbable, that he had laboured under some bodily disorder, which occasioned his going to Bath for the benefit of the waters, which in the time of the Romans were in so high esteem. And the good success, which he met with by the use of them, may be concluded from the tenor of the inscription, wherein he makes his acknowledgement to the deities above mentioned, for the benefit he had received thro their favour, in consequence of his addresses to them for that purpose. For as it was a common notion of the antient pagans, that all human affairs were under the direction of their deities; so in any danger or misfortune they used to solicit them for relief, with vows and promises of erecting altars and other buildings to their honour, in case of a favourable answer. Which, when performed, they were said *Votum solvere*, as the letters v. s. here imply.

II. The other inscription, on the lower altar, when expressed in words at length, may be read in the following manner :

*Sulevis Sulinus Scultor, Bruceti filius, sacrum fecit libens merito.*

(1) *Brit. Rom. Ind. cap. 1.* Spon has published a small treatise upon this subject, with the following title: *Ignotorum atque obsecratorum deorum arae. Studio Jacobi Sponii. Lugd. 1676, 12mo.*



That the first word *SVLEVIS* denotes a name given to certain rural goddesses, called *Sulevæ*, is plain from an inscription found on a stone at Rome, and published by Fabretti, in which they are joined with *Campestræ*. Most of those female deities, which consisted of a plurality, whether benevolent or hurtful, are commonly represented as three in number. Such were the *Eumenides*, *Gorgons*, *Graces*, *Harpyes*, *Heesperides*, *Sirens*, *Sibylls*, and some others mentioned by Fabretti (1). And agreeably to this notion three female figures in a group, cut on stone, have been found in three different places at *Hornsteeds*, a Roman station in Northumberland, and published by Horsley (2). And tho no inscriptions now remain, to inform us whom they were designed to represent, the stones being very much broken; yet by their several attributes they all appear to be deities, or their attendants. But in the draught of the stone given us by Fabretti, three female figures are exhibited in a sitting posture, and under them three male figures standing, with an altar placed between them, and a swine prepared for sacrifice; and below this sculpture stands the inscription, in the words following:

SVLEVIS·ET·CAMPESTRIBVS·SACRVM  
L·AVRELIVS·QVINTVS·>·LEG·VII·X·GEMINAE  
VOTVM·SOLVIT·LAETVS·LIBENS  
DEDICAVIT·VIII·K·SEPTEMBRE·BRADVA·ET  
VARO COS (3)

---

(1) *De Aquis et Aquaeduct. in Græc. Thesaur. antiquitat. Rom. Tom. iv. p. 1733.*

(2) *Brit. Rom. Northumb. xlviii, xlix, l.*

(3) That inscription was afterwards republished by Spon, in his *Miscell. erudit. antiq. p. 107*; tho not altogether with his usual accuracy, as Fabretti elsewhere complains, *Inscript. antiq. et. p. 690.*

From this monument Montfaucon concludes, that as it contains only three female figures, the *Sulevæ* and *Campeſtres* were the ſame godeſſes; tho the origin of the former name, as he ſais, is not known (1). Nor indeed did the name itſelf, ſo far as I can find, any where appear, but in that inſcription, till the late diſcovery of this altar at Bath. And I cannot but think with that learned writer, that the ſame deities were intended by both thoſe appellations. Tho I do not apprehend, that *Sulevæ* and *Campeſtres* were in all reſpects ſynonymous names, and of equal extent in their ſignification; but that *Campeſtres* was the common name of thoſe rural godeſſes, who were diſtinguiſhed by particular titles in different places, where they were worſhiped as tutelar deities. Hence we have an altar in Gruter, inſcribed to them only by the general name of *Campeſtres*, without any additional title there given them (2).

The two next words SYLVINVS SCVLTOR muſt, I think, ſtand for the names of the perſon, who dedicated this altar; as the two following, BRVCETI F. acquaint us with that of his father. Had the word *Scultor* been ſo written thro a miſtake for *ſculptor*, and deſigned to ſignify his imploymēt; it ſhould, according to the uſual form of ſuch inſcriptions, have been placed after his father's name; as in this following given us by Reineſius: *Jovi Optimo Maximo, et Dianæ Victricæ Sanctæ, Lucius Valerius Lucii*

(1) *L' Antiq. explic. Tom. i. pag. 411. and Supplem. Tom. i. p. 236.*

(2) *Pag. MXV. Num. 2.*

*filius, Palatina* (tribu), *vascularius* (1), *dicat, dedicat* (2). Besides, it can scarce be supposed, that he could have been guilty of such a mistake, as to omit the letter *p* in writing the name of his own art, had that really been his intention.

The words *SACRVM FECIT*, in the last line, are of the same import with *dedicavit*; in which sense likewise *sacrum* alone is often used. And sometimes the reason of the dedication is added, as: *Sacrum, voto suscepto, fecit*, in Gruter (3). But that not being mentioned here must remain unknown.

There is nothing said in either of these inscriptions, which can afford any light towards settling the time, when they were erected. But so far as appears from the form of the letters, they may not improbably be supposed of somewhat a later date, than that mentioned above (4), as found near the same place.

(1) *Vascularius* was an artificer, who made vessels of any sort of metal, but chiefly of plate. *Cic. in Verr. iv. cap. 24. Leg. 20. §. 2. D. de praecript. verb.*

(2) *Clafs, 1. Numb. 258.*

(3) *Pag. lxxxii. 1. and MLXXII. 8.*

(4) *See pag. 285.*

XLVII. *An Account of a remarkable Echinus: By Gustavus Brander, Esq; F. R. S.*

Read Dec. 11, 1755. **T**HIS echinus is of a very singular species. It appears to me to be of a middling nature between the echinus and the star-fish. It comes from the island of Bourbon in the East-Indies; and I cannot learn, that it is any where described. (PLATE VIII. Fig. 3.)

XLVIII. *An Account of the Impression on a Stone dug up in the Island of Antigua, and the Quantity of Rain fallen there for Four Years: In a Letter from the Reverend Mr. Francis Byam to William Fauquier, Esq; F. R. S.*

Dear Brother, Antigua, March 31, 1755.

Read Dec. 11, 1755. **A**S you have the honour to be a member of the Royal Society, I have sent you, by Captain Barrett, in a box directed for you, what I esteem to be a great curiosity. It is a stone, that was brought from a quarry, for a building in the town: the quarry is in the side of a mountain, and is about three hundred yards higher than high-water mark, and about two miles from the sea. When the mason struck it with his hammer, it split in two, and discovered the exact portraiture of a fish (on each stone) which we call an old wife. (PLATE IX.) I have likewise sent you an account of the quantity of rain, that has fallen in this place for four years last past.

past. Had I had such an instrument, as Mr. Derham had, to measure the hundredth part of an inch, it would have increased the quantity greatly; but I have only taken notice of it when it amounted to the tenth part of an inch. My instrument is very simple and plain; it is a tin vessel about thirteen inches high, the aperture at top is four inches and an half square, the bottom the same; the opposite sides parallel to each other. It stands in my garden, free from the drippings of any trees or houses, and raised upon a pedestal about two feet from the ground. You will observe a  $\times$  in September 51, and September 54. In these months we had hurricanes. If any thing of this kind would be agreeable to you, or the Society, I shall take great pleasure in communicating them, when in my power. I am, dear brother,

March 31, 1755.

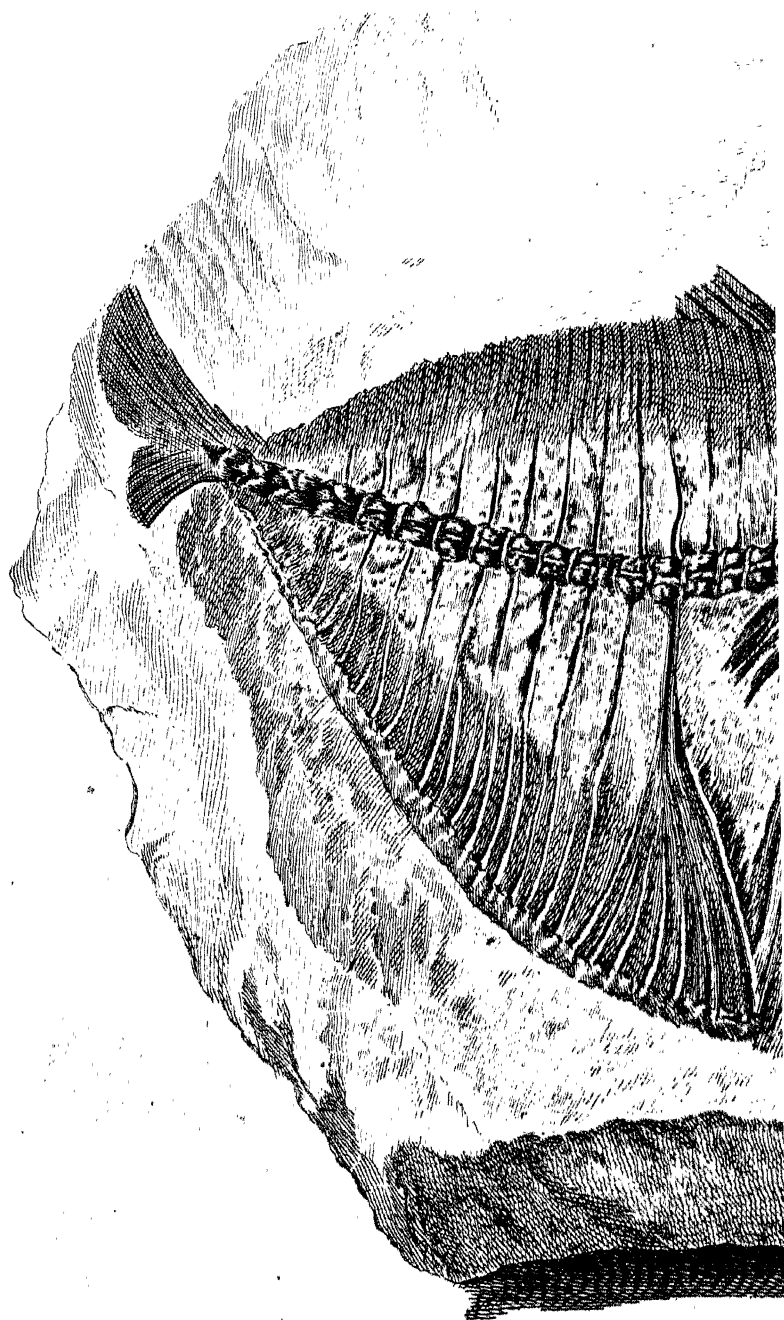
Your affectionate brother,

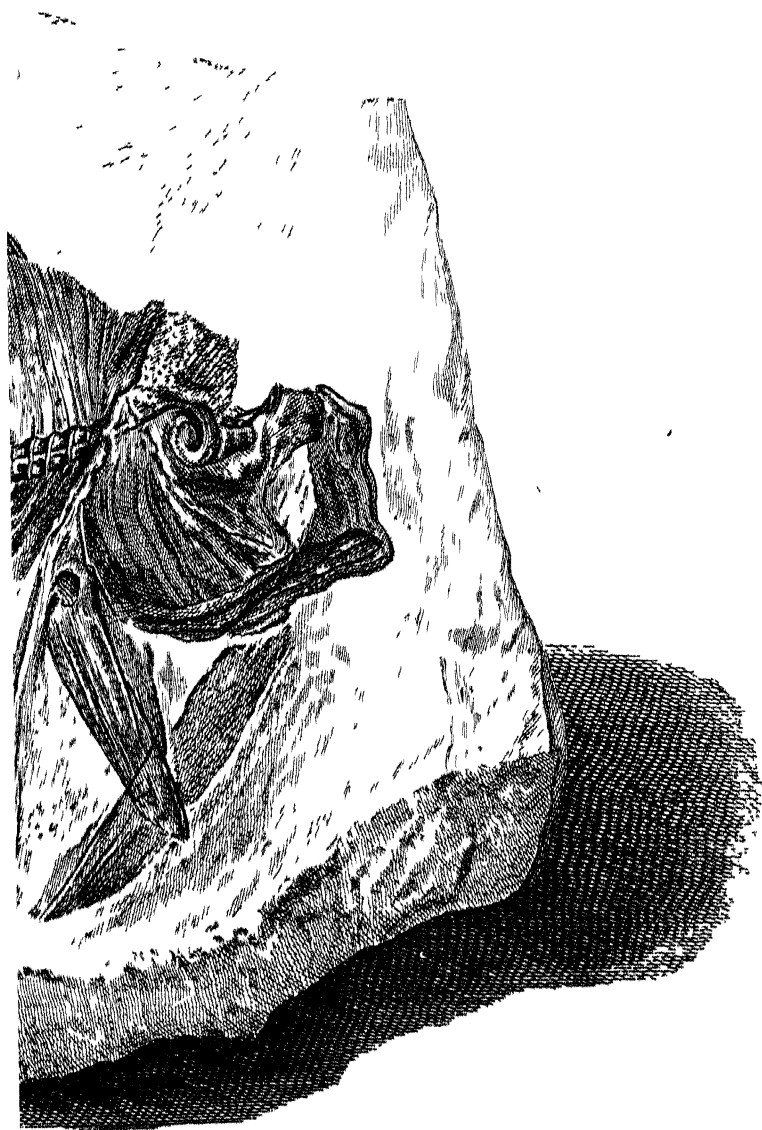
F. Byam.

A TABLE of the Rain, which has fallen in Antigua,  
from 1751 to 1754.

1751	Inch.	Tenths	1752	Inch.	Tenths	1753	Inch.	Tenths	1754	Inch.	Tenths
Jan.	2	6	Jan.	0	6	Jan.	3	1	Jan.	0	8
Feb.	1	5	Feb.	0	0	Feb.	1	7	Feb.	1	0
March	2	1	March	3	2	March	3	2	March	2	8
April	2	5	April	0	0	April	2	2	April	2	1
May	7	2	May	0	0	May	2	1	May	6	3
June	4	3	June	8	7	June	2	3	June	9	9
July	8	4	July	2	9	July	2	8	July	6	3
Aug.	3	6	Aug.	10	0	Aug.	5	1	Aug.	7	0
Sept. $\times$	6	2	Sept.	5	6	Sept.	3	9	Sept. $\times$	14	0
Octo.	4	8	Octo.	4	8	Octo.	4	3	Oct.	11	2
Nov.	3	9	Nov.	2	8	Nov.	0	8	Nov.	6	6
Dec.	4	7	Dec.	4	7	Dec.	1	3	Dec.	7	2
<hr/>			<hr/>			<hr/>			<hr/>		
51			8	43			3	32			8
<hr/>			<hr/>			<hr/>			<hr/>		
									75		
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**XLIX.** *A Letter to the Right Honourable George Earl of Macclesfield, President of the Royal Society, concerning the Stones mentioned in the preceding Article. By Mr. Arthur Pond, F. R. S.*

My Lord,

Read Feb. 5,  
1756.

**A**S your Lordship was pleased to inform me, that the council had thought fit to order Mr. Byam's account of the fossil fish, found in Antigua, to be printed, which curious specimen my worthy and most esteemed friend William Fauquier, Esq; was kindly pleased to add to my collection; I have made a drawing of it, that a print, the best illustration of the account, may accompany it, and have taken the liberty add a short description of the colours, and some other particulars, which will not be apparent in the print.

The impressiion of this fish is in a chalky kind of stone, of a pale ochrey colour; some parts, when scraped, are white, and all the impressiion is of a yellowish brown, nearly the colour of brown ochre.

The impressiions of the bones and fins are very perfect; and the cavity, that contained the back-bone, extremely sharp and delicate. When I first saw it, two or three of the vertebræ were in it. All the cavities of the bones are now sufficiently open to contain them, and 'tis probable, that most, if not all of them, were in the stone, when it was first split.

Between the rib-bones and the two long fins, which come down from the head, which parts were

only fleshy, there is not any impression, the stone having united quite through; and on the upper part of the fin, by the side of the cheek, is a deep impression of a very small cockle-shell. The impression on the counter-part of the stone is much the same, except that the tail is wanting.

The forms in this fish are so very delicate and elegant (as indeed they are in all the works of nature), that I do not pretend to have done them justice; but I have been particularly careful not to exaggerate or add the smallest trifle, by way of making it seem more complete or perfect than it is. I am, with the greatest respect,

Your Lordship's

Great Queen-street,  
Feb. 5, 1756.

most obedient and

most devoted servant,

Arthur Pond.

L. *An Account of the Effects of Lightning in the Danish Church, in Wellclose-Square. By Gustavus Brander, Esq; F. R. S.*

Read Dec. 18,  
1755.

HAVING been informed, that the lightning some time since had done damage to the Danish Church in Wellclose-square, Ratcliff-highway, I waited on Mr. Michelson the minister, who lives in the square, to know the particulars, and who very obligingly informed me,

me, that on Monday the 17th past, between six and seven o'clock, there was, among many others, one most amazing flash, accompanied with a clap of thunder, that equalled in report the largest cannon: That the next morning, observing the church-clock to be silent, they went into the bellfry, and found the wire and chain, that communicated from the clock in the bellfry to the clapper in the turret, where the bells hangs, to be melted; and that the small bar of iron from the clock, that gives motion to the chain and wire, just where the chain was fastened, was melted half through, the bar being about three-fourths of an inch broad, and half an inch thick. Several links of the chain, and of the wire, I have now the honour to shew you, when it will be observed, that the lightning took effect only in the joints. But whether it entered by communication from the wire exposed to the air in the small turret, through the roof of the bellfry, or at the windows, there being several panes broke in the south and west corners, I cannot say; although I presume rather the first way, as it is very possible, that the bare report of the thunder might have occasioned the latter.

The pieces of the wire and chain were scattered over the whole bellfry, nor could it be discerned, that the wood-work, or aught else, had suffered.

LI. *Electrical Experiments, made in pursuance of those by Mr. Canton, dated Decem. 3, 1753; with Explanations by Mr. Benjamin Franklin, Communicated Mr. Peter Collinson, F. R. S.*

Philadelphia, March 14, 1755.

*Principles.*

Read Dec. 18, 1755. I. **E**lectric atmospheres, that flow round non-electric bodies, being brought near each other, do not readily mix and unite into one atmosphere, but remain separate, and repel each other.

This is plainly seen in suspended cork balls, and other bodies electrified.

II. An electric atmosphere not only repels another electric atmosphere, but will also repel the electric matter contained in the substance of a body approaching it; and without joining or mixing with it, force it to other parts of the body, that contained it.

This is shewn by some of the following experiments.

III. Bodies electrified negatively, or deprived of their natural quantity of electricity, repel each other, (or at least appear to do so, by a mutual receding) as well as those electrified positively, or which have electric atmospheres.

This is shewn by applying the negatively charged wire of a phial to two cork balls, suspended by silk threads, and by many other experiments.

*Prepa-*

*Preparation.*

Fix a tassel of 15 or 20 threads, 3 inches long, at one end of a tin prime conductor; (mine is about 5 feet long, and 4 inches diameter), supported by silk lines.

Let the threads be a little damp, but not wet.

*Experiment I.*

*Pass an excited glass tube near the other end of the prime conductor, so as to give it some sparks, and the threads will diverge.*

Because each thread, as well as the prime conductor, has acquired an elastic atmosphere, which repels, and is repelled by, the atmospheres of the other threads: if those several atmospheres would readily mix, the threads might unite, and hang in the middle of one atmosphere, common to them all.

*Rub the tube afresh, and approach the prime conductor therewith, crossways, near that end, but nigh enough to give sparks; and the threads will diverge a little more.*

Because the atmosphere of the prime conductor is pressed by the atmosphere of the excited tube, and driven towards the end where the threads are, by which each thread acquires more atmosphere.

*Withdraw the tube, and they will close as much.*

They close as much, and no more, because the atmosphere of the glass tube, not having mix'd with the atmosphere of the prime conductor, is withdrawn  
intire,

intire, having made no addition to, or diminution from, it.

*Bring the excited tube under the tuft of threads, and they will close a little.*

They close, because the atmosphere of the glass tube repels their atmospheres, and drives part of them back on the prime conductor.

*Withdraw it, and they will diverge as much.*

For the portion of atmosphere, which they had lost, returns to them again.

### Experiment II.

*Excite the glass tube, and approach the prime conductor with it, holding it across near the opposite end, to that on which the threads hang, at the distance of 5 or 6 inches. Keep it there a few seconds, and the threads of the tassels will diverge. Withdraw it, and they will close.*

They diverge, because they have received electric atmospheres from the electric matter before contained in the substance of the prime conductor; but which is now repelled and driven away, by the atmosphere of the glass tube, from the parts of the prime conductor, opposite and nearest to that atmosphere, and forced out upon the surface of the prime conductor at its other end, and upon the threads hanging thereto. Were it any part of the atmosphere of the glass tube, that flowed over and along the prime conductor to the threads, and gave them atmospheres (as in the case when a spark is given to the prime conductor, from the glass tube), such part of the  
tube's

tube's atmosphere would have remained, and the threads continue to diverge; but they close on withdrawing the tube, because the tube takes with it *all its own Atmosphere*, and the electric matter, which had been driven out of the substance of the prime conductor, and formed atmospheres round the threads, is thereby permitted to return to its place.

*Take a spark from the prime conductor, near the threads, when they are diverged as before, and they will close.*

For by so doing you take away their atmospheres, composed of the electric matter driven out of the substance of the prime conductor, as aforefaid, by the repellency of the atmosphere of the glass tube. By taking this spark you rob the prime conductor of part of its natural quantity of the electric matter; which part so taken is not supplied by the glass tube, for when that is afterwards withdrawn, it takes with it its whole atmosphere, and leaves the prime conductor electrified negatively, as appears by the next operation.

*Then withdraw the tube, and they will open again.*

For now the electric matter in the prime conductor, returning to its equilibrium, or equal diffusion, in all parts of its substance, and the prime conductor having lost some of its natural quantity, the threads connected with it lose part of theirs, and so are electrified negatively, and therefore repel each other, by *Pr. III.*

*Approach the prime conductor with the tube near the same place as at first, and they will close again.*

Because



Because the part of their natural quantity of electric fluid, which they had lost, is now restored to them again, by the repulsion of the glass tube forcing that fluid to them from other parts of the prime conductor : so they are now again in their natural state.

*Withdraw it, and they will open again.*

For what had been restored to them is now taken from them again, flowing back into the prime conductor, and leaving them once more electrified negatively.

*Bring the excited tube under the threads, and they will diverge more.*

Because more of their natural quantity is driven from them into the prime conductor, and thereby their negative electricity increased.

### Experiment III.

*The prime conductor not being electrified, bring the excited tube under the tassel, and the threads will diverge.*

Part of their natural quantity is thereby driven out of them into the prime conductor, and they become negatively electrified, and therefore repel each other.

*Keeping the tube in the same place with one hand, attempt to touch the threads with the finger of the other hand, and they will recede from the finger.*

Because the finger being plunged into the atmosphere of the glass tube, as well as the threads, part of its natural quantity is driven back through the

hand and body, by that atmosphere, and the finger becomes, as well as the threads, negatively electrified, and so repels, and is repelled by them. To confirm this, hold a slender light lock of cotton, two or three inches long, near a prime conductor, that is electrified by a glass globe, or tube. You will see the cotton stretch itself out towards the prime conductor. Attempt to touch it with the finger of the other hand, and it will be repelled by the finger. Approach it with a positively charged wire of a bottle, and it will fly to the wire. Bring near it a negatively charged wire of a bottle, it will recede from that wire in the same manner, that it did from the finger; which demonstrates the finger to be negatively electrified, as well as the lock of cotton so situated.

*LII. Extract of a Letter concerning Electricity, from Mr. B. Franklin to Monsr. Delibard, inclosed in a Letter to Mr. Peter Collinson, F. R. S.*

Philadelphia, June 29, 1755.

Read Dec. 18, 1755. **Y**OU desire my opinion of Pere Beccaria's Italian book. I have read it with much pleasure, and think it one of the best pieces on the subject, that I have seen in any language. Yet as to the article of water-spouts, I am not at present of his sentiments; though I must own with you, that he has handled it very ingeniously. Mr. Collinson has my opinion of whirlwinds and

water-spouts at large, written some time since. I know not whether they will be published; if not, I will get them transcribed for your perusal. It does not appear to me, that Pere Beccaria doubts of the *absolute impermeability of glass* in the sense I meant it; for the instances he gives of holes made through glass by the electric stroke are such, as we have all experienced, and only shew, that the electric fluid could not pass without making a hole. In the same manner we say, glass is impermeable to water, and yet a stream from a fire-engine will force through the strongest panes of a window. As to the effect of points in drawing the electric matter from clouds, and thereby securing buildings, &c. which, you say, he seems to doubt, I must own I think he only speaks modestly and judiciously. I find I have been but partly understood in that matter. I have mentioned it in several of my letters, and except once, always in the *alternative*, viz. that pointed rods erected on buildings, and communicating with the moist earth, would either *prevent* a stroke, *or*, if not prevented, would *conduct* it, so as that the building should suffer no damage. Yet whenever my opinion is examined in Europe, nothing is considered but the probability of those rods *preventing* a stroke, or explosion; which is only a *part* of the use I proposed from them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, tho' of equal importance and advantage.

I thank you for communicating M. de Buffon's relation of the effect of lightning at Dijon, on the 7th of June last. In return give me leave to relate an instance I lately saw of the same kind. Being in  
the

the town of Newbury in New England, in November last, I was shewn the effect of lightning on their church, which had been struck a few months before. The steeple was a square tower of wood, reaching seventy feet up from the ground to the place where the bell hung, over which rose a taper spire, of wood likewise, reaching seventy feet higher, to the vane or weather-cock. Near the bell was fixed an iron hammer to strike the hours ; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and thro' a second floor in like manner ; then horizontally under and near the plaistered cieling of that second floor, till it came near a plaistered wall ; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square, in which the church stood, so that nothing remained above the bell.

The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them, except making the gimlet-holes, through which the wire passed, a little bigger, and without hurting the plaistered wall, or any part of the building, so far as the aforesaid wire and the pendulum wire of the clock extended ; which latter wire was about the thickness of a goose quill. From the end of the pendulum down quite to the ground the building was exceedingly rent and damaged, and some stones in the foundation-wall torn out, and thrown to the distance

of twenty or thirty feet. No part of the aforementioned long small wire, between the clock and the hammer, could be found, except about two inches, that hung to the tail of the hammer, and about as much, that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gun-powder is by common fire, and had only left a black smutty track on the plaistering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the cieling, under which it passed, and down the wall. These were the effects and appearances: on which I would only make the few following remarks; *viz.*

1. That lightning, in its passage through a building, will leave wood to pass as far as it can in metal, and not enter the wood again till the conductor of metal ceases.

And the same I have observed in other instances, as to walls of brick or stone.

2. The quantity of lightning, that passed through this steeple, must have been very great by its effects on the lofty spire above the bell, and on the square tower all below the end of the clock pendulum.

3. Great as this quantity was, it was conducted by a small wire and a clock pendulum, without the least damage to the building so far as they extended.

4. The pendulum rod being of a sufficient thickness, conducted the lightning without damage to itself; but the small wire was utterly destroyed.

5. Though the small wire was itself destroyed, yet it had conducted the lightning with safety to the building.

6. And

6. And from the whole it seems probable, that if even such a small wire had been extended from the spindle of the vane to the earth, before the storm, no damage would have been done to the steeple by that stroke of lightning, though the wire itself had been destroyed.

LIII. *A Letter concerning the Effects of Lightning at Darking in Surrey, from Mr. William Child to Mr. James Pitfold. Communicated by Mr. Peter Collinson, F. R. S.*

S I R,

Darking, Sept. 30, 1755.

Read Dec. 18,  
1755.

**A**FTER several disappointments of sending you the account I so long ago promised, you have here the best particulars I can obtain. The day, on which the storm happened, was Monday the 16th of July 1750, about seven o'clock in the evening. During the preceding part of the day the air was of a very red fiery appearance, accompanied with frequent thunderings. About six o'clock the wind arose, and blew exceeding strong, and in a very short time the hemisphere became uncommonly dark; the flashes of lightning were much stronger, and came in very short intervals of time, and the thunder-claps long and loud, attended with a very hard rain for near half an hour, in which time came the strongest flash of lightning I ever saw, and instantly with it the most terrible burst of thunder.

der. I was then in an open shop, not more than 200 yards from Mr. Worsfold's house, where it did the damage. Several persons, who were nearer, saw at the same time, in different places about the house, large balls of fire, which, as they fell upon the houses or ground, divided into innumerable directions. I should have mentioned, that the wind was southward during the whole storm.

The lightning entered Mr. Worsfold's house upon the south side of the roof, close in a small angle of a stack of chimneys, that stand out several feet above the tiling, and falling perpendicular through the roof, met with a small crank, which was in a passage between the north and south chambers: To which crank hung a bell, and from the crank went a wire both ways into the two chambers. It ran along the wire, that went into the back or south chamber, melting it to the end, and, when it left it, split the post of a bed, that stood in the chamber, as though it had been cleft with wedges. It followed the course of the other wire into the north chamber, which turned towards the east, and went partly round the room, following its direction in every angle where the wire went \*, till it reached the end, which was joined by a string, to which hung a handle for ringing the bell, it being close by the side of the bed: but the greatest force of the lightning seemed to fall perpendicular down the side of a wall in the chamber. Against the chimney were hung several barometers, the glasses of which were all shattered to pieces, and

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\* These wires conducting the lightning, as far as they went, confirms Mr. Franklin's opinion, that if they had been extended to the earth, the great damage, that ensued, might have been prevented.

forcing away the plaistering of the wall, entered the shop, piercing through the two upper shelves, and the parcels of nails, &c. that were upon them. And here it is observable, that from the perpendicular course it took the same direction in the shop, as in the chamber over it, but in almost as many lines as there were shelves, leaving very visible marks of its course. Near its perpendicular course in the shop, upon one of the shelves, it pierced through seven box-irons, making a small hole about the bigness of common shot on one side, and leaving a roughness on the opposite side of each box where it came out. The several parcels of nails, tacks, hinges, &c. that lay in the course it took, were very plainly affected by it: some of the small tacks in particular were soldered together, six, seven, eight, or ten in a clump, as if they had had scalding metal run over them. The papers of the parcels were burnt in small holes. At one end of the shelves hung several long pendulums, the springs of which were melted so that they fell to the ground; and the lightning spreading its remaining force to some littered straw and packing-paper, that lay about the shop, set fire thereto, which was happily extinguished without doing any further damage. Mr. Worsfold was in his shop the whole time, but received no hurt.

This, Sir, is the best account I can procure; from which if you can reap any satisfaction, I shall think myself happy in having it in my power to oblige you. I am

Darking, Sept. 30,  
1735.

Your most humble servant,

W. Child.

LIV.



LIV. *An Account of the great Benefit of blowing Showers of fresh Air up through distilling Liquors.* By Stephen Hales, D.D. F. R. S.

Read Dec. 18,  
1755.

THE great importance of having a sufficient supply of fresh water in ships has been the occasion of many laudable attempts to make sea-water fresh and wholesome: but all the attempts and discoveries hitherto made have laboured under this great and material objection, *viz.* the great quantity of fuel, that was necessary to distil, with a slow progress, a small quantity of water, by any methods of distillation hitherto known. But I have lately happily, most unexpectedly, discovered an easy and effectual method to distil great quantities of water with little fuel; which I was led to by the following incidents; *viz.* Mr. Shipley, secretary of our Society *for the encouragement of arts, manufactures and commerce*, brought me acquainted with Mr. William Baily of Salisbury-court, the author of many ingenious contrivances; who shewed me, in a small model of a tin vessel, a method, by which he has happily increased the force of the engine to raise water by fire, *viz.* by lifting up some of the boiling water, at every stroke, by means of a conical vessel, with small holes in it, full of tow; whereby the quantity of the ascending steam or wreak was considerably increased. This led me to think, that a greater quantity of liquor might also by this means be distilled; but on trial I found the  
increase.

increase to be only one twelfth part, though considerable in the expanded form of a steam. Hence I was led to try what would be the effect of causing an incessant shower of air to ascend through the boiling liquor in a still; and this, to my surprize, I found on trial to be very considerable. There was another circumstance also, which probably conduced to lead my mind to this thought, *viz.* About six months before, Mr. Littlewood, a shipwright at Chatham, came thence purposely to communicate to me an ingenious contrivance of his, soon to sweeten stinking water, by blowing a shower of fresh air through a tin pipe full of small holes, laid at the bottom of the water. By this means, he told me, he had sweetened the stinking bilge water in the well of some ships; and also a butt of stinking water in an hour, in the same manner as I blew up air thro' corn and gunpowder, as mentioned in the book on *Ventilators*.

2. The method, which I used to blow showers of air up through the distilling water, was by means of a flat round tin box, six inches diameter, and an inch and half deep; which is placed at the bottom of the still, on four knobs or feet half inch high, to make room for the liquor to spread over the whole bottom of the still, that the heat of the fire may come at it. In larger stills this box must be proportionably larger, and have higher feet. And whereas the mouth of the still is too narrow for the tin box to enter, which box ought to be within two inches as wide as the bottom of the still; therefore the box may be divided into two parts, with a hinge at one edge or side, and a clasp at the other, to fix it together,

ther, when in the still. This box must be of copper for distilling sea-water ; mine was made of tin, for other liquors also. The air-pipe, which passes through the head of the still, will help to keep the air-box from moving to and fro by the motion of the ship ; or, if that should not be found sufficient, three or four small struts may be fixed to the sides of the air-box. They must reach to the sides of the still. The cover and sides of the air-box were punched full of very small holes, one-fourth inch distant from each other, and about the twentieth part of an inch in diameter. On the middle of the cover or lid of this air-box, was fixed a nosil more than half inch wide, which was fitted to receive, to put on, and take off the lower end of a tin pipe, which was twenty inches long, and passed through a hole in the head of the still : four inches of the upper end of this pipe were bent to a crook, almost at a right angle to the upright stem, in order thereby to unite the crook to the widened nose of a pair of kitchen double bellows, by means of a short leathern pipe of calves-skin. See PLATE X. *Fig. 1.* This tin air-box, and many more of them for other persons, were made by Mr. Tedway, tinman, over-against the Meuse-gate, Charing-cross.

3. The double bellows were bound fast to a frame, at the upper part of the iron nose, and at the lower handle, in order the more commodiously to work them. And that the upper half of the double bellows may duly rise and fall, in order to cause a constant stream of air (besides the usual contracting spiral springs withinside), several flat weights of lead must be laid on the upper part of the bellows,  
near

near the handle, with a hole in their middle, to fix them on an upright iron pin fastened on the bellows; that by this means the weights may the more commodiously be put on or taken off. For, according to the different depths of the liquor in the still, so will the force of the included air, against the upper board of the bellows, be more or less. Thus, supposing the depth of the water in the still to be twelve inches, from the surface of the depressed water in the air-box, then the pressure of the included air against the upper part of the bellows, will be equal to that of a body of water a foot deep, and as broad as the inner surface of that board. It will, therefore, be requisite to add or take off weights, according to the different depths of the water in the still, at different periods of the same distillation. The bellows must be proportionable to the size of the still, but need not be very large. Where-ever the stills are fixed in ships, the air may be conveyed to them from the bellows, either through a small leathern pipe, distended with spiral coiles of wire, or through Bamboo canes, or broad small wooden pipes, like hollow fishing rods.

4. When I first distilled in this ventilating way, in order to estimate, what the difference might be in the quantity distilled, by that or the common method, I tried both ways, by receiving the distilled liquor into a quarter of a pint glass, estimating the times by a pendulum beating seconds. Where I found, to my surprize, that sometimes three times more was distilled by ventilation than by the usual way: But finding inequalities in the small quantities thus distilled, in order the more fully and assuredly

to ascertain the true proportion there was in the two methods of distilling, I put three gallons of water into the still ; and, when it boiled, put on the still-head, and fixed its nose to the worm-tub pipe ; which tub was full of cold water. When it had distilled for an hour, the receiver was instantly taken away. And on measuring the distilled water, found it to be two quarts and 45 cubic inches by a glass divided into cubic inches. And a gallon containing 282 cubic or solid inches, this quantity of distilled water, which was 186 cubic inches, is  $\frac{2}{3}$ th part of a gallon.

5. Then, filling the still as full of water as before, and when it began to boil, fixing the head to the still and worm-tub, which was full of cold water ; there was distilled in an hour, with constantly blowing showers of fresh air up through the stilling liquors, five quarts, less by seven cubic inches, which is 345.5ths cubic inches ; that is, little less than the double of the quantity, that is distilled in the usual way. In several other distillations of a quart at a time, I found the quantity distilled by ventilation to be more than the double of that in the usual way. So that the quantity by ventilation may, at a medium, be estimated the double of the usual distillation. It is the well known property of moving air, to carry along with it a considerable quantity of adjoining vapour, as also of falling water to carry much air down along with it.

6. It is to be hoped therefore, that so considerable an increase in the quantity distilled will be of great benefit to navigation, as it may be done in less time, and with less ~~fire~~.

7. In

7. In the account of Mr. Appleby's process, for making sea-water fresh, which is published by order of the lords of the admiralty, in the Gazette of Jan. 22, 1754, it is said, that a still, which contains 20 gallons of water, will distil 60 gallons in ten hours, with little more than one bushel of coals; and therefore 120 gallons in 20 hours, with little more than two bushels of coals. And by ventilation 240 gallons, or a tun; and 24 gallons may be distilled in twenty hours, making an allowance for the times of heating those stills full of cold water; and a still something larger and wider will distil a tun in 24 hours; which will more than suffice for a sixty gun ship, with 400 men, whose provision of water for four months is about 110 Tuns. And larger ships may either have proportionably larger stills, or else two of them. As for merchant-ships with few men, a small still will be sufficient.

8. The second-sized stills contain 10 gallons, and will produce 60 gallons in 20 hours, with half the above-mentioned fuel; and by ventilation 120 gallons.

9. The least stills contain five gallons, and will produce 32 gallons in 20 hours; and by ventilation 64 gallons in 20 hours.

10. I have seen some of these stills at Messrs. Steel and Stephens's, over-against Mercers-chapel, in Cheapside, which have been made for this purpose. There are holes in the feet of the iron frame or stove, to screw them down to the deck. They were fixed at the fore-castle before the mast, in King Charles the second's time, when they thought they had discovered the way to distil sea-water, free from the noxi-

ous

ous spirit of salt, and from the nauseous bitter taste. Or, if it be thought proper, one part of the ship's boiler may be made use of, by adapting a still-head to it.

11. Now supposing a still to contain 25 gallons, and that four parts in five of it, *viz.* 20 gallons are distilled off: then, in order to distil a tun, or 210 gallons, the still must be emptied, cleansed, and re-filled eleven times; and if the whole be done in 24 hours, full 16 of those hours will be taken up in distilling at the rate of a gallon in about four minutes and an half; and the remaining eight hours of the 24, being divided into 11 equal parts, they will be each near 44 minutes to empty and cleanse the still, to refill it, and give the sea-water a proper boiling distilling heat: whether this can be done in so short a time, must be known by experience, and ought therefore first to be tried at land.

12. Doctor Butler, in his lately published method of *procuring fresh water at sea*, proposes the pouring in more sea-water into the still, thro' a funnel fixed in a small hole in the head or upper part of the still, when more than half the former water is distilled off; by which means the water in the still will soon acquire a distilling heat; and this to be repeated several times; but then it will be requisite to add each time more chalk, in such proportion as shall be found requisite. It will be well to try this method in hopes thereby to increase the quantity of water that is distilled. The hole in the head, or upper part of the still, is to be stopped with a small plate of copper, so fixed as to turn to and from over the hole.

[ 13. Doctor

13. Doctor Butler used capital soap-lees, in the proportion of a wine quart to 15 gallons of sea-water, which sufficed for four or five times repeated pourings-in of more sea-water into the still. But as I have found, that a small quantity of chalk has the same good effect, and is cheaper, and more easily to be had, it is therefore preferable to soap-lees.

14. When there is a fire in the cook-room, the sea-water might be ready heated to put into the still, without any additional expence of fuel, in the following manner, which I shall here describe; tho' I think it probable that it will not be put in practice; yet, as farther improvements may possibly hereafter be made in it, and as it may be of use in some cases, at least, I shall here give an account of it, *viz.*

15. About the year 1718, Mr. Schmetou, a German gentleman, got a patent here for heating great quantities of water, with little expence of fuel, which he then shewed me. Having fixed a spiral iron worm-pipe, in such a brick stove or chimney as women heat their irons in, thereby causing the water to run from a vessel, through the worm-pipe, several feet length round, in the fire. About thirty years after I acquainted Mr Cramond of Twickenham with this, as hoping it might be of benefit in distilling sea-water. Upon which he procured such a spiral iron worm-pipe, which was about twenty feet long, and six-tenths inch diameter; the diameter of the spiral coile was about fourteen inches.

16. This I fixed in a brick stove in my garden, with its upper end fixed to a vessel, which contained 45 gallons of water. I found the event of this first

trial.



trial to be as follows, *viz.* When the water run full bore, at the rate of a gallon in 17 seconds, the heat of the water was found, by a mercurial thermometer held in the stream, at the lower end of the pipe, to be 80 degrees above the freezing point, 180 degrees being the heat of boiling water. When, by means of a turn-cock, a gallon of water was two minutes in running, then the heat was 140. At which rate the 45 gallons would be an hour and half in running through the iron pipe; at which rate 25 gallons will run through in 50 minutes, with so considerable a degree of heat; and if it was an hour running, the heat would approach still nearer to a boiling heat, when first put into the still, which would forward the distillation, if wanted.

17. I pumped the heated water up again into the upper vessel; and thus continued to circulate the heating water, till its heat was 160 degrees in the upper vessel, *viz.* within 20 degrees, or one-ninth of boiling, the heat requisite for plentiful distillation. I was in hopes that if the water in the upper vessel could have been brought to a due degree of heat, and a still-head were fixed on it, with its cooling worm-tub, then water might have been distilled in ships, by having the iron worm-pipe fixed in the chimney of the cook-room: But I found, that when the heat of the water in the upper vessel was 160 degrees, *viz.* within one-ninth of boiling; then, in running through the iron worm-pipe again, it was so over-heated as to expand in the pipe, into an explosive vapour, which hindered the running of the water. However I thought it not improper to give an account of this attempt, notwithstanding it failed.

Not

not knowing whether this method of heating water may not in some cases, at land at least, be of use, thereby to save, in some degree, both fuel and time : Perhaps an iron worm-pipe of a larger bore might do better.

18. The waste of fuel will be less in proportion to the quantity distilled in large, than in small stills ; and the wider the still-head is, so much the more liquor will be distilled, and more with a worm-tub than without it. The worm-tub may be so covered as to prevent the flowing over of the water by the motion of the ship.

19. It is of great importance to take care to keep all parts of the still clean, that there may be no rust or verdigrease in the copper, which will occasion vomiting.

20. If it be necessary, the better to close the joining of the still head, it may be done with a lute or paste, made of a mixture of powdered chalk and meal, wetted with salt-water.

21. Now that several effectual means are discovered, to make distilled sea-water wholesome, and also to distil it in much greater quantity in the same still, in the same time, and with nearly the same quantity of fuel ; it is reasonable to believe, that it will be of great benefit to navigation, not only in saving much stowage room, for other important purposes ; but also in procuring fresh sweet wholesome water, instead of stinking putrid water, hitherto used ; which must needs have a tendency to promote that putrid distemper, the scurvy. And if withal due care be taken to exchange for fresh air the putrid close confined air of ships, which has occasioned the

death of millions of mankind; then navigation will become remarkably more healthy, and with little more danger to health and life than at land, except from storms.

22. Now supposing, that, in a sixty gun ship, the 110 tuns of water, for four months use, were distilled at the expence of three bushels of coals to a tun, this would consume nine chaldrons of coals: And as a chaldron of coals weighs about a tun and half; hence it appears, that coals will distil about eight times their quantity of water. And the 110 tuns of water weighing (at the rate of 2240 pounds to the tun) 138 tuns; and the nine chaldrons of coals weighing thirteen tuns and half, that is 94 tuns and half less than the 110 tuns of store water; and allowing twenty-four tuns and half for the still, water-casks, and coals, there will be 70 tuns weight of stowage saved thereby, for other uses. Or if some tuns of store-water are carried by way of precaution, which it will be adviseable to do, especially at first, till they can be assured, by repeated experience, what quantity can be depended upon by distillation; even then about half the tunnage will be saved, which will be a very material advantage.

23. Though when the distilling liquor runs from the bottom of the worm-pipe, through a long pipe fixed to it, the waste by the ventilating rushing air is not great, when the water in the worm-tub is not hot; yet the following precaution, if needful, may be used, in distilling by ventilation, *viz.* to fix at the lower end of the worm-pipe, by means of a wooden faucet, a small cask for a receiver; the faucet to enter the upper side of the head of the cask,  
and

and in order to give a free passage for the great quantity of ventilating air to pass off, and withal, at the same time to prevent the escaping of much moist vapour with it, it will be proper to fix at the bung-hole a long upright pipe of wood, or of any metal. I used a gun-barrel four feet and a half long; thro' which some small degree of moist vapour escaped; as appeared by the dampness of a piece of paper, fixed at a little distance above the mouth of the gun-barrel. This vapour became visible, and much increased, when the water in the worm-tub was very hot; at which time, less is distilled into the cask-receiver; then also there is more danger of the spirit of salt arising. And it was observable, that the water in the worm-pipe vessel heated much sooner by ventilation than in the common way of distilling. For which reason that water ought to be changed so much the oftener, which can easily be done at sea. The cocks also at the side of the worm-tub ought to be large, in order to let the hot water off the faster.

24. But though the water in the worm-tub was sooner heated by ventilation, because a double quantity of hot steam passed through it more than passed through it in equal times in the common way of distilling; yet in the usual way of distillation the liquor in the still is hotter, with equal fire, as is evident by its aptness to boil over through the worm-pipe; whereas in the ventilating way it did not boil over, notwithstanding a very hot fire was purposely made for a trial. The continual streams of ascending fresh air, not only in some degree abating the heat of the water, but also incessantly carrying off the more rarefied particles of the water, which, when expanded into a repelling state,

do thereby cause the overflowing ebullition of the water. On which account it is probable, that less spirit of salt is formed and raised by ventilation than without it. As also on account of the fresh air ascending, not from the bottom of the still, where is the greatest plenty of salt, especially towards the latter end of each distillation; but about three inches from the bottom, *viz.* principally from the many holes at the surface of the air-box.

25. And whereas the quantity raised from the still, and distilled into the cask-receiver, cannot be seen; the proper quantity to be distilled in each distillation may with great accuracy be known, by having a well-closed pewter bottle, of the size of about half a pint, with a brass wire as big as a goose quill, fixed to it, the wire to pass through the receiver-cask, near the bung-hole, which the floating pewter bottle will raise up, till the marks on the wire appear just above the cask. I made use of a glass phial for this purpose. This wire will rise and fall freely, notwithstanding the motion of the ship, if it passes not only through the wood of the cask, but also through a metalline pipe two or three inches long, fixed in that hole. And it will be known by the simmering or boiling noise of the water in the still, whether it is hot enough to distil; for the running of the water into the receiver-cask cannot be seen.

26. As it might be suspected, that more spirit of salt would be raised, and distilled over in the ventilating way, than without it; having procured 18 gallons of sea-water by the Margate hoy, which was taken up at some distance from the shore, I put three  
gallons

gallons of this sea-water, as soon as I had received it, into the still; and when it began to distil, air was blown up through it. For some time, as is usual, in the distillation of sea-water, no spirit of salt arose; but after distilling some time longer, there were very weak whitish clouds, with drops of solution of silver in aqua-fortis, as in the common way of distilling. Hence we see, that ventilation does not increase the quantity of salt, but rather probably somewhat decreases it, for the reasons given, N<sup>o</sup> 24.

27. I distilled three gallons of sea-water, which had stunk, and became sweet again; when about ten quarts of it had been distilled off, then there began to be very weak whitish clouds with solution of silver, but none with solution of mercury; which shews the water to be hitherto good, agreeably to what I formerly had found to be the good effect of distilling sea-water, which had putrified, and become sweet again; of which I published an account in the year 1739. But when I continued the distillation on, a quarter of an hour longer, *viz.* till there was but a pint of water remaining in the still, and the salts were incrusted on its sides, up near three inches from the bottom, and lay in heaps at the bottom of the still, then the distilled liquor had whitish clouds in it, with the solution of mercury in aqua-fortis. From this distillation we see, that putrefaction, by dissolving the bitter salt and bitumen, into very minute parts, qualified them to combine with the more fixed common salt, so as to detain them from rising in distillation.

28. I distilled three gallons of sea-water, with the proportion of six ounces of Mr. Appleby's lapis infernalis,

fernalis, and fix ounces of calcined bones to twenty gallons of sea-water, as he directs. This water lathered well with soap, and boiled peas well.

29. I distilled also some sea-water with half an ounce of stone-lime to a gallon, from the Clee hills in Herefordshire, which having been preserved ten months in a firkin, had flacked to dry powder. This distilled water did also lather well with soap, and boiled peas well; which proves that the lime, which is a fixed body, does not distil over with the water. Since I made this distillation, General Oglethorpe informed me, that his father, Sir Theophilus, told him, that lime was one of the ingredients, which he and the rest of the patentees, in Charles the second's time, called the cement, with which they made distilled sea-water wholesome.

30. I distilled also some sea-water with the like proportion of powdered chalk, which boiled peas well, and was better tasted than the waters distilled with lapis infernalis, or lime. I distilled also some sea-water with an ounce of chalk to a gallon, but found no difference in the taste of this, and that which had but half an ounce of chalk to a gallon: So that half an ounce of chalk to a gallon of water will be sufficient; but where the sea-water is saltier, or more bituminous, more chalk may be added if needful.

31. Dr. Alston, of Edinburgh, in the preface to the second edition of his *Dissertation on Quick-lime and Lime-water*, says, That “ the like effect was  
“ found in distilling sea-water with lime, that it  
“ neither precipitated a solution of silver in aqua-  
“ fortis, nor a solution of corrosive sublimate in wa-  
“ ter,

“ter, nor did it form a pellicle of various colours  
 “on its surface, as did the water distilled by Mr.  
 “Appleby’s process”. And I find, page 35, of my  
 book on this subject, that lime of oyster-shells had  
 the same good effect, but required two distillations;  
 I might then use too small a quantity of that lime.  
 Hence it is probable, that the chalk, the lime, the  
 lime in the lapis infernalis, and the lime in Dr. But-  
 ler’s soap-lees, seize on and fix not only the bittern  
 salt, but also the bitumen of the sea-water, as we  
 learn from the like effect in the purification of the  
 salt of hartshorn. That the saline spirit arises chiefly  
 from the bittern salt, and not from the more perfect  
 sea-salt, is probable from hence, *viz.* When I di-  
 stilled three gallons of common water, made as salt  
 as sea-water with common salt; no spirit of salt  
 arose, even though the distillation was carried so far  
 as to leave the salt, though very damp, to lie in  
 heaps, and it was incrusted on the sides of the still,  
 for about three inches from the bottom.

32. It is a considerable further advantage, that  
 water thus distilled by ventilation, being thereby re-  
 plete and freshened with air, has for present use a  
 more agreeable taste than water distilled without ven-  
 tilation, which requires the standing a longer time  
 to have its more disagreeable adust taste go off. And  
 as the volatile oil of pepper-mint does arise on the  
 wings of the ventilating air during the distillation;  
 so also may that part of the bitumen, which is vola-  
 tilized by heat; as also the volatile urinous salts of  
 the sea-water, which arise from animal substances,  
 be sublimed in the same manner.



33. It was observable, that the water distilled fast, even though the water in the still was below the surface of the tin air-box, through which the greatest part of the ascending shower of air rushed. Hence the ventilating air, in ascending among the vapours, carries them off fast. Hence it is to be suspected, that this method of ventilation will not do well for simple waters, or fermented vinous spirits; because they being very volatile, much of them may be carried off in waste.

34. It was observable, that in these distillations of sea-water, no whitish clouds appeared on dropping in solution of corrosive mercury, not even when considerably more than four parts in five of the water had been distilled over. And it was the same with the mixture of lapis infernalis, lime, and chalk; whence it is probable, that the lime and chalk seize on and fix the more volatile bitter salt, as does also the lime in the lapis infernalis. And it is well known, that sugar, that sweet salt, cannot be made without lime, on which, as its centre of union, it fixes and granulates.

35. And whereas with a solution of silver in aqua fortis, which was much weakened and diluted with water, there appeared a faint degree of whitish cloud, in all the above-mentioned distillations, though not with the stronger solution of mercury, till the distillation was carried on, much beyond four parts in five of the water in the still; when both solutions caused remarkably white clouds, especially the solution of mercury; which indicates the quantity of the spirit of salt which was raised during the former part of the distillation to be exceeding small, since it could  
not

not seize on, nor disengage the aqua-fortis from the stronger solution of mercury, though it did in a very small degree in the weak solution of silver, so as to let loose a very little of the silver, which thereby caused the faint clouds. When a drop of the solution of mercury was dropped into the distilled water, after a drop of the solution of silver, it reformed the silver cloud, and made the water clear, by means of the great proportion of acid aqua-fortis that was in it.

36. Now in order to make some estimate of the very small quantity of spirit of salt in these several distilled waters, I dropped a drop of the solution of silver into an ounce, or 480 grains of pure rain water, which gave no clouds; but on dropping in a drop of sea-water, which weighed a grain, the white clouds were strong. And since sea-water can dissolve nine times more salt than it has in it; therefore, supposing the drop to be so fully impregnated with salt, then the salt would be the 480th part of the ounce of water. But as there is nine times less salt, therefore the proportion of the quantity of spirit of salt will be but the 4320th part. And how much less must be the proportion of salt in these distilled waters, which is not sufficient to make a sensible impression on solution of mercury, and but a faint one on much diluted solution of silver? Such distilled sea-water will not therefore probably be unwholesome; almost all spring-waters have some degree of salt in them: But if there were more of the spirit of salt, a very small quantity of pot-ash, or pearl-ashes, or salt of tartar, combined with it, will turn it into common salt, the quantity of which would be extremely little.

37. It may be well to be provided in ships with some silver dissolved in aqua-fortis, mixed with pure rain-water, or distilled fresh water, in the proportion of sixty drops to an ounce of the water: though it is probable it may seldom be wanted, unless in some doubtful cases, when the taste may not be accurate enough to perceive, whether there be any spirit of salt in the distilled water.

38. Since double the usual quantity of vapour may by way of ventilation be carried off, common salt may thus be made much sooner, cheaper, and better; because, as there is much less fire used, so proportionably, less of the fine acid spirit of the salt, in which its virtue consists, will be evaporated away: For it is well known, that the salt is best, which has undergone the least action of fire in making.

39. This more speedy method of evaporating will also be useful, in making many other evaporations; as in making pot-ash, &c.

40. But some are apprehensive, that this great improvement in distilling may be of ill consequence in making those destructive spirits cheaper, which are already but too cheap. Had not the improvement been of great benefit to mankind in many other respects, I should have been far, very far, from endeavouring after it, or discovering it. But should the event be to make those spirits cheaper, and consequently, by spreading farther, more destructive, the consequence of that will be, that the increased raging devastation will the sooner necessarily rouse the nations to put a stop to what must be done hereafter: for if the ravages continue increasing, as they have done for sixty years past, the human

man species must needs not only be greatly debased, but even in great measure diminished and destroyed. And yet none of the nations, whose very vitals are thereby consuming and destroying, endeavour to put any stop to it, except the heads of the native Indians in North-America, who have long repeatedly intreated the English to sell them no rum; which is as effectually extirpating of them, as the hornet did the unsubdued remainder of the Canaanites.

41. If mankind, instead of receiving and entertaining this pest with almost universal applause and approbation, could prevail with themselves to be in earnest, to use means to deliver themselves from it, then much might be done towards it, by lowering and weakening all kind of fermented distilled spirits with water, to a salutary degree, as is now practised in our plantations in America, in making punch so weak, as not to be hurtful; which, when it was much stronger, was well known to destroy multitudes. And where the like humane, wise, and laudable practice has been used in ships, it has had the same happy salutary effect.

42. What necessity or even temptation can there be to be averse to the making them wholesome, instead of being venomous and destructive? and that not only of the lives, but even of the morals of mankind. How much therefore does it behove all, who have any concern for the honour and dignity of their own kindred species, any indignation at its being thus debased and disgraced, any bowels of pity for the vast multitudes, not less than a million, that are yearly destroyed all over the world, by this moral as well as natural, and therefore worst of all evils

that ever befel unhappy man, to use their utmost endeavours to deliver mankind from this pest? But notwithstanding this astonishing ravage and destruction of the human species, yet the unhappy unrelenting nations of the world seem as unconcerned about it, as if only so many thousands, nay, millions of caterpillars or locusts were destroyed thereby. Was there ever a more important occasion to rouse the indignation of mankind? Can we be calm and undisturbed, when this mighty destroyer rears up its invenomed head every-where? The most zealous advocates for drams, even the unhappy besotted dramists themselves, the prolonging of whose lives, and whose real welfare both here and hereafter is hereby sincerely intended, cannot find fault with this well-meant remonstrance, in defence of them, and of all mankind, against this mighty destroyer, from one, who has long been labouring, and that not without success, in finding means to preserve multitudes of lives, by various ways.

*IV. An Account of the great Benefit of Ventilators in many Instances, in preserving the Health and Lives of People, in Slave and other Transport Ships. By Stephen Hales, D. D. F. R. S.*

Read Dec. 18, 43. <sup>1755.</sup> **I**T is to be hoped, that the several means here proposed for having fresh and sweet water at sea, will be of great benefit in preserving the health and lives of multitudes of that valuable

able and useful part of mankind, those, who occupy their business in great waters; whose welfare I have long had at heart, and endeavoured to promote by various ways; especially by finding means to procure them fresh salutary air, instead of the noxious, putrid, close confined pestilential air, which has destroyed millions of mankind in ships. And it is to be hoped that, by diligent researches, farther and farther useful discoveries will hereafter be made for the benefit of navigation.

44. The following, as they are strong proofs of the great benefit and usefulness of ventilators in ships, so they also fully prove, that they can most commodiously be fixed and worked in them, in contradiction to the vulgar, false, and groundless notion, that they take up too much room, and are incommodious, and in a manner impracticable to be worked, whereas the men are eager to work them; and many more persons can be with safety to their health and lives in a ventilated, than in an unventilated ship; which fully obviates the objection as to the room they take up. In new and important researches, the likeliest way to succeed, is to pursue a thought, not only by imperfect and fallacious reasonings, but, when the nature of the thing requires it, with a proper series of trials and experiments. Thus, in the present case, the principal cause of the sickness in ships is the noxious putrid air; the obvious remedy is the exchanging that foul air for fresh, by effectual means, which are seldom discovered by dwelling only on objections, but are usually the reward of repeated, diligent, experimental researches. Neither are we to be discouraged in these our pursuits by some disappointments,

ments, for I have frequently found that they lead to the thing fought for. And by the like clue of reasoning and experimenting, there is the greatest probability, that we shall succeed in another very important research, *viz.* the preserving much longer from decay the timbers of ships laid up in ordinary in harbour. For, as we are assured by daily experience, that the decay is wholly owing to damp, close, confined, putrid, corroding air; so the only remedy for this evil is the frequently changing the air among the timbers, by plentiful ventilations; which, we find, by happy experience, can be effected to such a degree, as gives reasonable hopes, enough to encourage our farther trials and researches.

45. Captain Thomson, of the Success Frigate, in his letter to me, dated London, Sept 25, 1749, says,  
 “ That, during the ventilation, the lower-deck  
 “ hatches were commonly kept close shut; by  
 “ which means the air<sup>u</sup> was drawn down into the  
 “ hold, from between the decks, through the seams  
 “ of the ceiling, along the timbers of the ship; by  
 “ which means we found the foul air soon drawn  
 “ off from between decks. Our rule for ventilat-  
 “ ing was for half an hour every four hours: but  
 “ when the ventilating was sometimes neglected for  
 “ eight hours together, then we could perceive, espe-  
 “ cially in hot weather, a very sensible difference by  
 “ that short neglect of it; for it would then take a  
 “ longer time to draw off the foul air. Our general  
 “ rule was, to work the ventilators till we found the  
 “ air from them sweet. We all agreed, that they  
 “ were of great service; the men being so sensible  
 “ of the benefit of them, that they required no  
 “ driving

“ driving to work that, which they received so much  
 “ benefit by. We found this good effect from ven-  
 “ tilation, that though there were near 200 men on  
 “ board, for almost a year, yet I landed them all  
 “ well in Georgia, notwithstanding they were pressed  
 “ men, and delivered me out of gaols, with di-  
 “ stempers upon them. This is what I believe but  
 “ few transports, or any other ships, can brag of;  
 “ nor did I ever meet the like good-luck before;  
 “ which, next to Providence, I impute to the bene-  
 “ fit received by the ventilators. It is to be re-  
 “ marked, that we, who lay wind-bound, for four  
 “ months, with our expedition fleet, which soon  
 “ after invaded France, were very healthy all the  
 “ time, when they were very sickly in all the ships  
 “ of that expedition.

46. “ This certainly occasioned all kind of grain  
 “ provisions to keep better and longer from wec-  
 “ vels than otherwise they would have done; and  
 “ other kinds of provisions received benefit from the  
 “ coolness and freshness in the air of the ship, which  
 “ was caused by ventilation.’

47. Mr. Cramond also informs me, that he found  
 the good effect of ventilators on board a slave-ship  
 of his with 392 slaves, twelve of which were taken  
 on board, just before they sailed from Guinea, ill of  
 a flux, which twelve all died; but the rest, with all  
 the Europeans in the ship, arrived well at Buenos  
 Ayres.



*The following is a Letter to me, from Captain Ellis, viz.*

S I R,

48. “ COULD any thing increase the pleasure I  
 “ have in a literary intercourse with you, it  
 “ would be to find, that it answered your end in pro-  
 “ moting the public good. The *Vis inertæ* of  
 “ mankind is not the only difficulty you have had  
 “ to encounter, but their ignorance and prejudices,  
 “ which are almost insuperable. It is to your per-  
 “ severance and resolution, that the little progress  
 “ you have made is due: Indeed I ought not to say  
 “ little; for it is a great step to have found the few,  
 “ that have hearts good enough to relish your plan,  
 “ and heads sufficiently clear to discern the most  
 “ effectual method of advancing it. It does honour  
 “ to those noble and other worthy personages, that  
 “ join you in acts of such extensive humanity, as  
 “ the introduction of ventilators to hospitals, prisons,  
 “ ships of war and transport, &c. as they must ne-  
 “ cessarily render the miseries of the first more sup-  
 “ portable, and the close and constant confinement  
 “ of the others less prejudicial and fatal to their health  
 “ and life. It is to be lamented, that they are not  
 “ more generally made use of; for, notwithstanding  
 “ their advantage is apparent and incontestable, it  
 “ is scarce credible how few are to be found among  
 “ the vast number of ships daily employed in car-  
 “ rying passengers, slaves, cattle, and other perish-  
 “ able commodities. Those of your invention,  
 “ which I had, were of singular service to us; they  
 kept

“ kept the inside of the ship cool, sweet, dry, and  
 “ healthy: The number of slaves I buried was only  
 “ six, and not one white man of our crew (which  
 “ was thirty-four), during a voyage of 15 months;  
 “ an instance very uncommon. The 340 negroes  
 “ were very sensible of the benefits of a constant  
 “ ventilation, and were always displeased when it  
 “ was omitted. Even the exercise had advantages  
 “ not to be despised among people so much confined.  
 “ I must not, however, forget, that ventilation alone  
 “ is insufficient to keep disorders out of ships; for  
 “ often infections are brought aboard by the slaves,  
 “ or others; and frequently diseases are produced  
 “ by feeding on bad or decayed food, but oftener  
 “ still by intemperance; for I have ever remarked, that  
 “ the immoderate use of spirituous liquors in warm  
 “ climates is more pernicious and fatal even than  
 “ the malignancy of the air itself. In cold Coun-  
 “ tries too, where I have had experience, those sail-  
 “ ors, or others, who accustomed themselves to hard  
 “ drinking, especially of drams, had the scurvy in  
 “ a terrible degree; whereas those, who were tem-  
 “ perate or sober, either escaped it entirely, or had  
 “ it but moderately. The effects of drunkenness  
 “ were still more discernable among the Indians ad-  
 “ joining our settlements in Hudson’s Bay, who are  
 “ a feeble, diminutive, chilly, indolent set of people.  
 “ On the contrary, those, who come from the inland  
 “ parts (who are unused to drink brandy), are brave,  
 “ active, robust, and industrious. The same dif-  
 “ ference is observable in the Africans, and perhaps  
 “ among the inhabitants of most other nations,  
 “ did we attend to it. It was to the unusual so-

“briety of my crew that I ascribed, in some measure, their uncommon healthiness; for sailors breathe a purer air, and enjoy more exercise and liberty, than passengers or slaves: wherefore their ailments are owing to bad or disorderly living, as well as to unwholesome air.

“Could I but see the immoderate use of spirituous liquors less general, and the benefits of ventilators more known and experienced, I might then hope to see mankind better and happier. I am,

“S I R,

Bristol, Dec.  
26, 1753.

“Your most obedient servant,

“Henry Ellis.”

49. And, by the like good conduct, in his next voyage in the year 1755, not one of 312 slaves died; and all his 36 sailors arrived alive and well at Bristol.

50. And the Earl of Halifax has often informed me of the great benefit they found by the use of ventilators, in several Nova Scotia transport-ships, twelve to one more have been found to die in unventilated than in ventilated ships. It is indeed a self-evident thing, that the changing the foul air frequently in ships, in which there are many persons, will be a means of keeping them in better health than not doing it; which makes it the more astonishing, that effectual proposals to remedy so great an evil should be received with so much coldness and indifference by mankind. They little consider, that

it is the high degree of putrefaction (that most subtile dissolvent in nature), which a foul air acquires in long stagnating, which gives it that pestilential quality, which causes what is called the gaol-distemper. And a very small quantity, or even vapour of this highly attenuated venom, like the infection or inoculation for the small-pox, soon spreads its deadly infection. Ought not men therefore, from the common natural principle of self-preservation, to use their utmost endeavours to shun this pestilent destroyer, by which millions of mankind have perished in ships? -

LVI. *An Account of some Trials to cure the ill Taste of Milk, which is occasioned by the Food of Cows, either from Turnips, Cabbages, or autumnal Leaves, &c. Also to sweeten stinking Water, &c. By Stephen Hales, D. D. F. R. S.*

Read Dec. 18, 51. 1755. **T**HIS method of blowing showers of air up through liquors will be of considerable use in several other respects, as well as in distillation, as appears by the following trials, *viz.*

51. I have been informed, that it is a common practice, to cure the ill taste of cream from the food of cows, by setting it in broad pans over hot embers or charcoal, and continually stirring it, till scalding hot, and till cool again. But when I at-

tempted to do this much sooner, and more effectually, by blowing showers of air up through it, I soon found it to be impracticable, by reason of its very great degree of frothing up. The ill taste must therefore be got out of the milk, before it is set for cream; which, I have been told, has been practised, and that with some benefit, by giving the milk a scalding heat, without stirring it.

53. May 22. I ventilated some ill-tasted new unheated milk of a cow, which was purposely fed with crow-garlick mixed with cut grass. After 15 minutes ventilation the taste was a little mended; in half an hour's blowing it was something better. At the hour's end it had the same taste, but was sensibly better than the unventilated milk. I was disappointed of an opportunity to repeat the experiment with crow-garlick milk, with a scalding heat: it would then probably have been soon perfectly cured; as it is reasonable to believe from the event of the following experiments, *viz.*

54. August 23, four quarts of ill-tasted new milk, from a cow, which had fed eighty-four hours on cabbage leaves only, and drank during that time very little water, were put into a leaden vessel, eight inches in diameter, and thirty inches deep. The leaden vessel was heated in a large boiler, and set into a vessel of hot water; thereby to give the milk a scalding heat, and also keep it hot. In ten minutes ventilation it was perfectly cured of its ill taste; and after standing twenty-four hours in a broad pan, there was a thick scum, which was half cream and half butter, free from any ill taste; the skimmed milk

milk was not sheer or thin : so here is a method to make good butter from ill-tasted milk.

55. The froth of the milk was so great, by reason of a too brisk ventilation, as to make it froth over the vessel, which was thirty inches deep ; if it had not been kept down, by constantly lading and breaking the very large bubbles of froth. But when the ventilation is more gentle, the froth has risen but three inches from six quarts of milk, which was nine inches deep. The cabbage milk was but six inches deep. I repeated the like operation the same day, with the evening milk of the same cow ; but giving it only a heat, that I could bear my fingers in, for a little time ; with this degree of heat, after forty-five minutes ventilation, the milk (though much better tasted), yet was not so completely cured as the former milk. Hence we see how necessary heat is, to volatilize the rancid oil (which gives the ill taste) to such a degree as to cause it to fly off by ventilation.

56. It was observed, that what was milked from this cow a week after she had done eating the cabbage, had an ill taste.

57. I have not as yet had an opportunity to try to cure, in the same manner, the ill taste of milk, which is occasioned by cows feeding on autumnal leaves, or turnips, they having probably eaten this autumn the fewer leaves, on account of the plenty of grass, occasioned by much rain ; which has hitherto prevented turnips from being rancid, which are observed to be most so when they shoot out in the spring. As opportunities offer I purpose to make trials, which I conclude others will also do, which will probably  
| be.

be attended with the same good effects as that on the cabbage milk.

58. But though the ill taste of milk from feeding on cabbage leaves was thus effectually cured by volatilizing with heat and dissipating by ventilation the rancid oil, yet the bitter taste of a strong infusion of chamomile flowers in six quarts of water was not sensibly abated by an hour's ventilation of it, while scalding hot.

59. I am informed, that, in Devonshire, they set the pans of milk on trivets, making fires under them, to give the milk, gently and gradually, a scalding, but not a boiling heat, which would disturb the rising cream; and then set it on the floor in the milk-house to cool, where in twelve hours it has a thick scum, partly butter and partly cream. The skimmed milk is very thin and sheer; and the cream in great plenty and delicious, except it gets a smoaky taste, which it is apt to do; and which might probably be prevented, by having a range of as many stoves as there are pans of milk to be used at one time; all to be warmed by one fire, either at one end, or the middle of the flue or funnel in the brick-work, which conveys the smoke and heat under the stoves. And as the pans nearest to the fire will soonest have their due heat, on their removal to bring the farthest and coolest pans nearest the fire; and instantly covering the uncovered stoves with proper covers to prevent the heat and smoke from coming out; by this means the milk would all be soon heated, with any kind of fuel, and that with much less in quantity than in the common way.

60. And the more effectually to prevent the smoke from coming at the milk, it may be well to have the broad outer rim of the pans turned perpendicularly downwards, three or four inches, that it may enter deep into a circular groove of sand; and if it shall be needful, the sand may be wetted in order the more effectually to prevent the passage of the smoke. I thought of this method about fifty years since, on tasting the smoaky butter in Somersetshire. By the same means the poor might save much fuel in boiling the pot, especially in summer, when a fire is wanted only for boiling the pot.

61. When any pans are to be removed from the stoves, the ascent of the smoke through the uncovered stove may be prevented by first closing the flue near the fire, by an iron sliding-shutter or register.

62. Milk might thus most commodiously be heated to a scalding heat with little fuel, fit for ventilation, in a vessel of a proper depth, set in the same manner as the pans in a stove, to secure it from smoke, with bellows fixed properly near it: (see *PLATE X. Fig. 3.*) By this means there would be little trouble or expence in curing ill-tasted milk by ventilation.

63. May 14th, merely to see what the event would be, a gallon of new milk just from the cow was ventilated, for an hour and half, which produced six ounces of butter; and though it was ventilated half an hour longer, yet no more butter was made; it was whitish, wanting both the colour and taste of good fresh butter.



64. I am credibly informed, that in the places famous for making the best fresh winter butter, they set the pot of cream in warm water, so long as till it has acquired that small degree of sourness, which it very soon has in warm summer weather, which gives it its agreeable flavour. And in order to give it colour, they grate a well-coloured carrot into a little milk, which, as soon as stained, is strained from the carrot through a sieve, and then mixed with the cream.

65. It is found by experience, that the quantity of cream is increased, by putting into the milk a little warm water in winter, and cold in summer; which being thereby in some degree thinned, the cream is thereby more easily disintangled, so as more freely to ascend to the surface of the milk.

66. I ventilated three gallons of stinking Jessop-well purging water. On first blowing, the smell of the ascending vapour was very offensive, which offensiveness abated much in five minutes: In eleven minutes the smell was much better: In twenty minutes the water seemed sweet both in smell and taste; and not sweeter at the end of forty-five minutes, fifteen or twenty minutes will probably suffice.

67. July 20th, three gallons of stinking sea-water were ventilated; in five minutes it was much sweetened, and no ill smell in the ascending air, though at first it was very offensive: At the end of ten minutes it had a small degree of ill taste; after twenty minutes no ill taste or smell. It frothed near a foot high during part of the ventilation: This from the bitumen, &c.

68. Some sea-water, which was made to stink with flesh and isinglass being put into it, was not made perfectly sweet, not even by a ventilated distillation, and an hour's more ventilation after it was distilled; so that putrefaction with animal substances is not easily completely cured by ventilation.

69. When the water was 27 inches deep in the leaden vessel, no air could be blown up through it by the force of the bellows. But at 18 inches depth the air could freely be blown up in showers, thro' the water; when therefore it is requisite to blow up through great depths of water, the bellows may be worked with a lever, as smiths bellows are worked.

70. As it is found by experience, that the milk and butter of cows, which drink stinking water, has a very bad taste, this plainly shews, that the water retains its putrid quality, when mixed with the blood. Whence it is much to be suspected, that the stinking water, which is drank in ships, by retaining its putrid quality, even when mixed with the blood, may thereby promote that putrid distemper the scurvy, as well as some other distempers. And much more does the putrid close air in ships, which is mixed with the blood from the lungs, promote putrid and other disorders. By the same means also pestilential infections are taken in: For as the salutary properties of good air are conveyed by the lungs, so are also the malignant qualities of bad air.

71. Thus also the putrid water in marshy aguish countries, may be a cause of agues, as well as the putrid air, which they breathe; which, as well as the putrid water, may probably carry some of its putrid quality into the blood through the lungs.

This method therefore of sweetening stinking water, by blowing showers of air up through the stinking water of some aguish places, may be beneficial.

72. Live fish may well be carried several miles, by blowing now and then fresh air up through the water, without the trouble of changing the water: for this ventilation will not only keep the water sweet, but also enrich it with air, which is necessary for the life of fishes; with which air they supply their blood, by breathing the water, thin spread, between their gills; but stinking water will kill fish.

73. I have found, that much of the heating oil may be got out of tar-water, by blowing showers of air up through it when scalding-hot, for 15 or 30 minutes, the longer the better; the less volatile, and more salutary acid remaining.

*Explanation of the Figures. PLATE X.*

*Fig. 1.* (*o o p r*) a tin or copper air-box, six inches diameter, and an inch and half deep from (*o* to *p*.)

The lid of the box full of holes, one twentieth inch diameter, and about a quarter of an inch distant from each other.

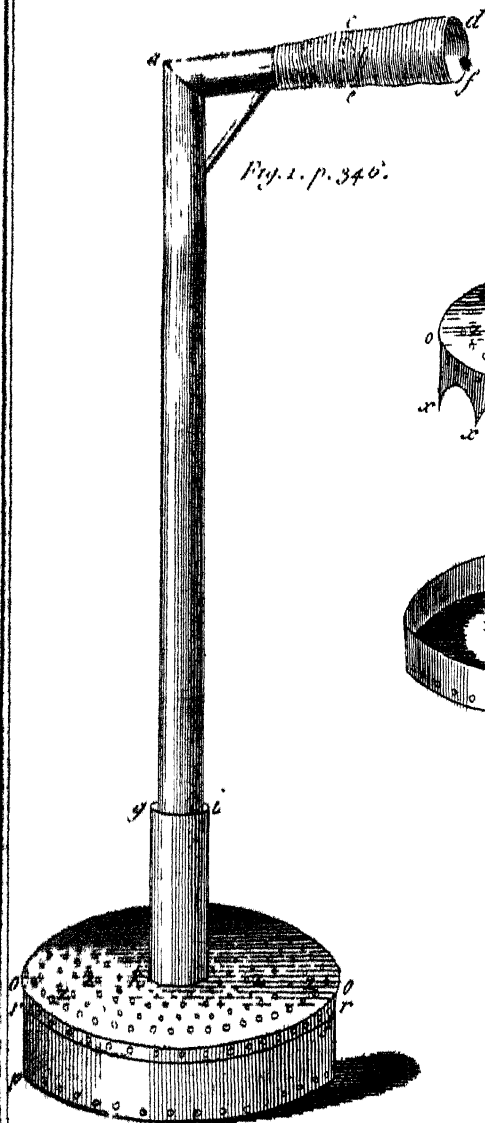
(*g i k l*) a nozel foldered to the lid of the air-box, into which the tin-pipe (*a g i k l*) is fixed so as to take in and out; this pipe to be two feet long, and six tenths inch diameter.

(*a b*) a bend in the pipe five inches long, to which is fastened the leathern pipe (*c c d f*) six inches long; to which the nose of the bellows is fixed at (*d f*).

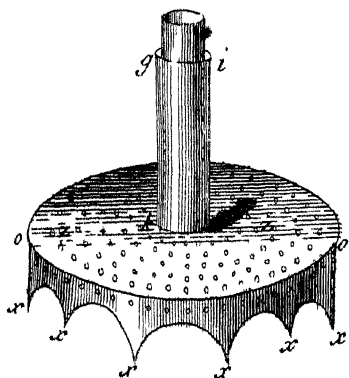
*Fig. 2.*



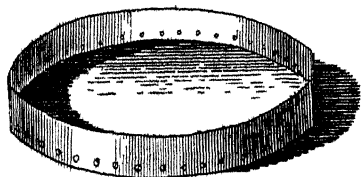




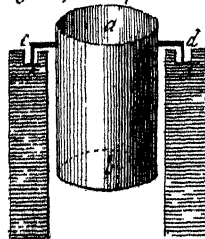
*Fig. 1. p. 346.*



*Fig. 2. p. 347.*



*Fig. 3. p. 347.*





*Fig. 2.* (*g i k l o o x x*) the lid of the box, whose rim (*o x o x*), is a quarter of an inch deeper than the box (*o p* Fig 1), that the air-holes (*o*) may be pierced in its upper-part; and the lower-part is scolloped with wide scollops, for the air to pass through the holes (*p p* Fig. 1.)

*Fig. 3.* (*a b*) the milk-boiler, with the broad rim (*c d*), and perpendicular rim (*c e d f*) foldered to the horizontal rim; the perpendicular rim to enter the circular groove (*e f*) four inches deep full of sand, thereby to prevent the ascent of the smoke from the fire-stove.

LVII. *Extract of a Letter of Thomas Barker, Esq; to the Reverend James Bradley, D. D. Astronomer Royal, and F. R. S. concerning the Return of the Comet, expected in 1757, or 1758.*

S I R, Lyndon, near Uppingham, Rutland, Dec. 17, 1754.

Read March 20,  
1755.

**A**S we expect the comet of 1531, 1607, and 1682, to return in 1757 or 1758, it is proper to be aware where to look for it. But that will be very different, according to the time of the year it comes; and its period is not sufficiently known to fix the month of its next perihelion, which should be July 25, 1757, according to its last period; but the length of that before would make it Oct. 25, 1758. I have therefore, in 12 short tables, given the apparent path of the comet, supposing its perihelion any month in



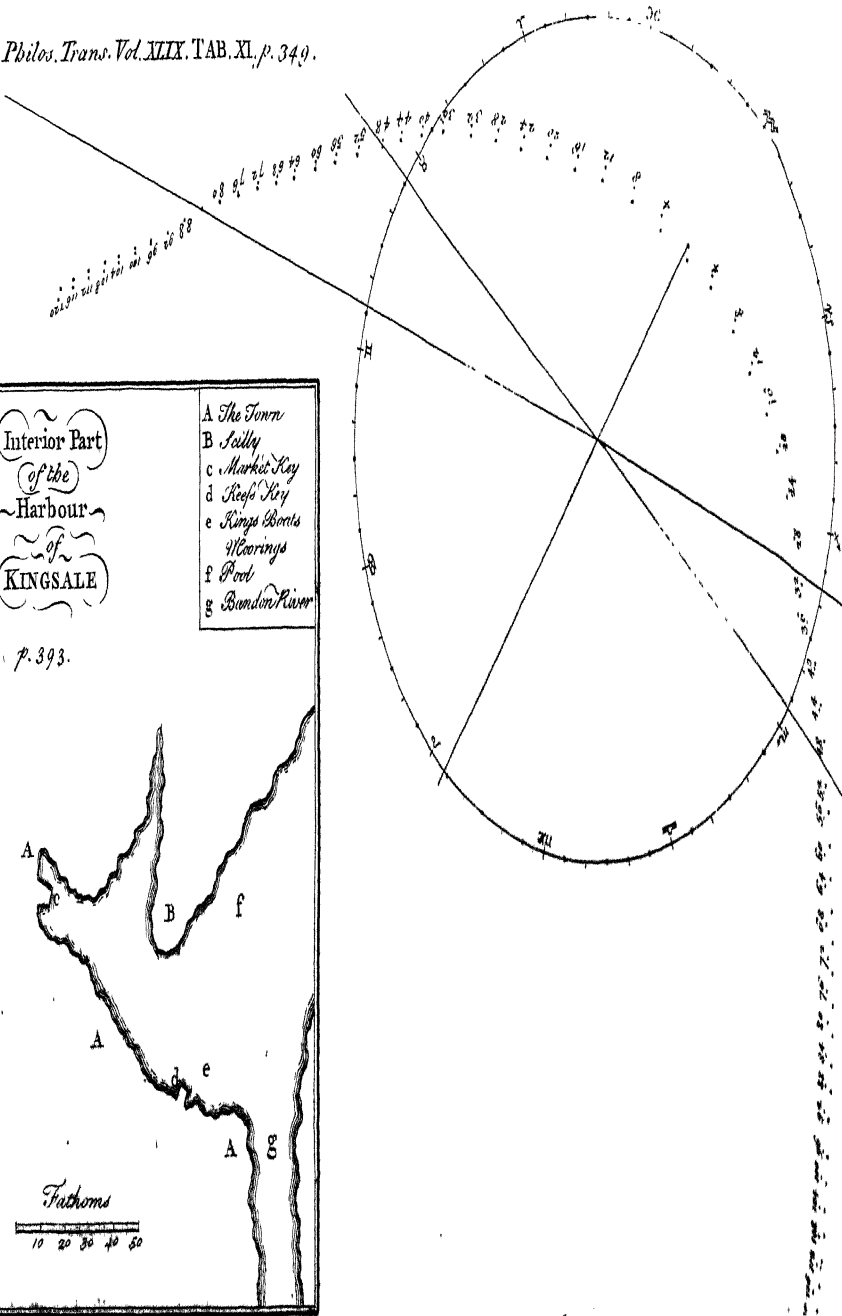
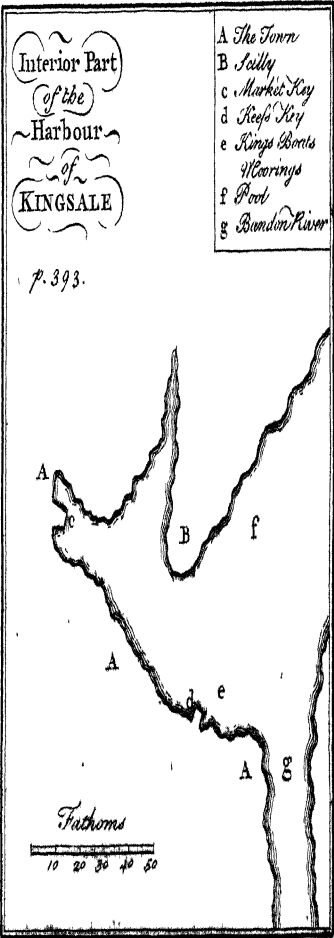
the year, with its curtate distance from the earth; and the two first articles of each are the places, which it would probably begin to appear in. These will shew in general the course of the comet, especially at its first appearance, which is most wanted; but cannot be depended on where its motion is swift, and may be  $40^{\circ}$  in a day, the beginning of May, or middle of October. From these tables, compared with the scheme, I have made another, where the comet would begin to be seen any month in the year.

To construct the places, on a large sheet of paste-board, I divided the circumference of a circle ten inches radius into degrees, for the magnus orbis. On the right point of the ecliptic and focal length I drew a parabola like that observed in 1682, round the sun, the center of the circle, and marked every fourth day's motion from the perihelion, and the line of its nodes. The co-sine of the comet's inclination set off on perpendiculars to this, towards the several points of the parabola, forms the projection of it, or points in the plane of the ecliptic, over which the comet is at any time perpendicular.

To find the comet's place at any time, count how long it is before or after its perihelion, and mark the place in the projection of the parabola: lay one edge of a parallel-ruler through that point, and the place the earth is then in, and the other edge passing thro' the sun, will cut the magnus orbis at the geocentric longitude of the comet: The tangent of the comet's inclination making the perpendicular from the comet's projected place to the line of nodes, the radius is the tangent of its apparent latitude, making the curtate distance of the comet from the earth the radius.









radius. For expedition thus ; draw two lines, making an angle of  $17^{\circ} 56'$  : on one of them set off the perpendicular from the comet's projected place, and raise a perpendicular to the other ; or, which is the same, from the comet's real place in the parabola ; and let fall a perpendicular, that is the tangent of the geocentric latit de.

One observation of a known comet will, on such a scheme, determine in some measure its whole course ; for, from the earth's place, draw the observed longitude of the comet, where that cuts the projection of the parabola is the comet's place ; to which if the observed latitude agrees, it confirms it : Then the other data being already known, and one place given, its whole course may be traced. Such a scheme may be also of use to find the periods of comets, where the description of one is not good enough to find its orbit by ; for if an old comet was seen in August, in  $\varpi$ , or in  $\S$ , with south latitude, or very bright in January, it cannot be the comet of 1682 ; but if in November in  $\gamma$ , near the ecliptic, it may. It then remains to see, whether the rest of the description will agree with the course it would in that case take : if it does, then, as the account is more or less perfect, there is a greater or less probability of its being the same. (See PLATE XI.)

A TABLE shewing here the Comet may be expected to begin to appear any Month.

Month	begin end	Scarce to be seen	Lat.	Time
January	begin	Retr. between 30° & 15'	Small increasing S.	7 Weeks after Perihelion
February	begin	30 & 15	Small N. or S.	a Month after Perihelion
March	end	30 & 0	Small N. decreasing	2 or 3 Weeks after
April	begin	15 & 0	Small N.	about Perihelion
May	end	Stat. 10 γ & 20 ✕	N.	1, 2, or 3 Weeks
June	begin	middle γ		
July	end	Dir. begin, γ		
August	begin	begin, γ		
September	end	end, γ		
October	begin	begin, II		
November	mid.	middle II		
December	end.	end II		
	begin	Stat. 25 & 30 II		
	end.	Retr. end II		
	begin	begin, II		
	mid.	5 II & 20 γ		
	end.	begin, γ		
	begin	begin, γ end.		
	end	begin, γ		

**LVIII.** *An extraordinary and surprising Agitation of the Waters, though \* without any perceptible Motion of the Earth, having been observed in various Parts of this Island, both maritime and inland, on the same Day, and chiefly about the Time, that the more violent Commotions of both Earth and Waters so extensively affected many very distant Parts of the Globe; the following Accounts, relating to the former, have been transmitted to the Society; in which are specified the Times and Places when and where they happened.*

L E T T E R I.

*From Mr. John Robertson, F. R. S. to Tho. Birch,  
D. D. Secret. R. S.*

At Portsmouth, in Hampshire.

S I R,

Read Nov. 27, 1755. **T**HE first notice I had of any uncommon motion of the waters in the docks at Portsmouth, was by the Whitehall Evening-Post; and as I did not hear any particulars from my acquaintance in the yard, I thought no more of it. But on the receipt of your letter I made a minute inquiry among the persons, who saw it, of whom there were many, and the substance of their information is as follows.

On Saturday, Nov. 1, 1755, about 35 minutes after ten in the morning there was observed in the

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\* See the note on the letter from R. Philips.



dock-yard at Portsmouth an extraordinary motion of the waters in the north dock, and in the bafon, and at two of the jetty-heads.

In the north dock, whose length is about 229 feet, breadth 74 feet, and at that time about 17½ feet depth of water, fhut in by a pair of ftrong gates, well fecured, his Majefty's fhip the Gosport of 40 guns, was juft let in to be dock'd, and well-ftay'd by guys and hawfers (certain large ropes fo called). On a fudden the fhip ran backwards near three feet, and then forwards as much, and at the fame time fhe alternately pitch'd with her ftern and head to the depth of near three feet; and, by the libration of the water, the gates alternately opened and fhut, receding from one another near four inches.

In the bafon, whose length is about 240 feet, breadth 220 feet, and at that time about 17 feet depth of water, fhut in by two pair of gates, lay the Berwick of 70 guns, the Dover of 40 guns, both in a direction nearly parallel to the Gosport; and a merchant fhip of about 600 tons, unloading of tar, lying in an oblique direction to the others. Thefe fhips were obferved to be agitated in like manner with the Gosport, and the tar-fhip to roll from fide to fide: The fwell of the water againft the fides of the bafon was obferved to be nine inches; one of the workmen meafured it between the librations.

The Naffau, a 70 gun fhip, lying along-fide a jetty-head, between the north dock and the bafon; alfo the Duke, a 90 gun fhip, lying againft the next jetty-head, to the fouthward, both in a direction nearly at right angles to the others, were obferved to  
be

be rock'd in the same manner, but not quite so violently: These two ships lay in the harbour.

I cannot learn, that the motion was observed in any other of the ships lying in the harbour, or by any person on land, not even by those, who stood near the borders of the dock, basin, and jetties, and saw the motion of the ships.

The dock and basin lie nearly east and west, on the west side of the harbour. I am,

Portsea, Nov.  
23, 1755.

S I R,

Your most humble

and obedient servant,

J. Robertson.

# L E T T E R. II.

*From Philip Carteret Webb, Esquire, F. R. S. to  
Thomas Birch, D. D. Secr. R. S.*

In Suffex, and the Southern Parts of Surrey.

S I R,

Bushbridge, Nov. 10, 1755.

Read Nov. 20, 1755. **Y**OU probably have had a circumstantial account of the extraordinary motion of the water in the dock and basin at Portsmouth, the first of November, between ten and eleven in the morning. An appearance something like it was observed about the same time, in several confined waters in different parts of Suffex and Surrey.

In my garden at Busbridge, near Godalmin in Surrey, on Saturday the first of November 1755, at half an hour after ten in the forenoon, Philip Smith, my gardener, and John Street, and John Johnson, two under-gardeners, were alarmed by a very unusual noise in the water, at the east end of the long canal, near which John Street and John Johnson were then at work. On their looking that way, they observed the water, in that part of the canal, in great agitation, attended with a considerable noise. The water soon raised itself in a heap or ridge, extending lengthwise about thirty yards, and between two and three feet above the usual level of the water; after which the heap or ridge heeled or vibrated towards the north, or left side of the canal, with great force, and flowed about eight feet over the grass walk on that side of the canal, quite up to the arch. On the water's returning back into the canal, it again raised itself into a heap or ridge in the middle; after which the heap or ridge heeled or vibrated with greater force towards the south, or right-hand side of the canal, and flowed over the grass walk, and thorough the rustic arch on that side; and drove a small stream of water, which runs thorough it, 36 feet back upwards, towards its source. During this latter motion, the bottom of the canal, on the north side, for several feet in width, was quite bare of water. The water being returned into the canal, the vibrations grew less and less, but so strong, as to make the water flow several times over the south bank of the canal, which is not so high as the north bank. In about a quarter of an hour from the first appearance the water became quiet and smooth as before. The motion of  
the

the water was, during the whole time, attended with a great perturbation of the sand from the bottom of the canal, and with a great noise, likened by the gardeners to that of water turning a mill. During the whole time the weather was remarkably still, there not being the least wind; and there was no tremor or motion of the earth felt on the sides of the canal. This is the substance of the account I collected from my gardeners; I being, when this appearance was observed, in London.

The canal is near 700 feet long from west to east, and is about 58 feet wide: there is a small spring; which constantly runs thorough it. The water at the east end, where this appearance was observed, usually pens from two to four feet, growing gradually deeper to the west end, where it pens to about ten feet. No motion was taken notice of in the water at the west end of the canal. The first vibration, which drove the water over the grass walks, was from south to north. The grass walk on the north side of the east end of the canal is 14 inches, and that on the south side about ten inches higher than the usual level of the water: the highest part of the walk, over which the water flowed, is about 20 inches above the water-level.

I am informed, that the water was affected about the same time in the following places.

In a mill-pond, at Medhurst in Suffex, the sudden agitation and swell of the water rolling toward the mill was so remarkable, that the miller imagined a sluice had been opened at the upper end of the pond, and had let a back-water into it; but upon search it was found to be shut as usual. Below the

mill the swell of the water was so great, as to drive the stream upwards, back into the conduit of the mill.

At Lee, in the parish of Whitley, in Surrey, about five miles from Busbridge, between Busbridge and Medhurst; the water in a canal or pond belonging to Mr. Luff was so violently agitated, that the gardener, on the first appearance, ran for help, thinking a number of otters were under the water, destroying the fish.

In a mill-pond, near Guilford in Surrey, a like swell and agitation of the water, I am told, was observed by several persons, one of whom stood all the time on a bridge of wood, over the pond. Not the least tremor or motion of the earth was, I am told, felt in any of these places, or at the bridge at Guilford. These are the circumstances I have been able to collect relating to this affair, which, if you think proper, you may communicate to the Society. I am,

Dear Sir,

Your most affectionate

and very obedient servant,

Philip Carteret Webb.

L E T T E R III.

*From Swithin Adece, M. D. F. R. S. to Philip Carteret Webb, Esq; F. R. S.*

S I R,

Guildford, Nov. 19, 1755.

Read Nov. 27, 1755. **I** Made inquiries at our mill, and of the neighbours near the river, but can hear nothing of the agitation of the water. Mr. Wright informs me, that he saw it in the back stream swell very considerably, and come with violence against the bank; but did not observe any sensible reflux; nor could he inform me in what direction the water moved. I am,

S I R,

Your most obedient humble servant,

S. Adece.

L E T T E R IV.

*From Swithin Adece, M. D. F. R. S. to Philip Carteret Webb, Esq; F. R. S.*

S I R,

Guildford, Nov 25, 1755.

Read Nov. 27, 1755. **I** Have met with a very particular account of the agitation of the water on the 1st of this month. An old sensible serious man, at Mrs. Willon's, in the parish of Cobham, was watering a horse in hand, at a pond close by the house, which is fed by springs, and had no current. The time he  
fixes

fixes was about ten in the morning, but their clock goes too slow. While the horse was drinking, the water run away from the horse, and moved towards the south with swiftness, and in such a quantity, as left the bottom of the pond bare, then returned with that impetuosity, which made the man leap backwards, to secure himself from the sudden approach of the water. It went back again to the south, with a great swell, and returned again. Upon inspecting the place, I found the water must have risen above one foot. The ducks were alarmed at the first agitation, and flew all instantly out of the pond. The man observed, that there was a particular calm at this time of day. You will observe here were two fluxes and two refluxes seen distinctly. This phenomenon will not be very surprising, since we have heard of the earthquake in Spain and Portugal. I am,

S I R,

Your most faithful humble servant,

S. Adee.

# LETTER V.

*From Mr. John Hodgson to Philip Carteret Webb,  
Esq; F. R. S.*

S I R,      Nov. 26, 1755, Six o'clock at night.

Read Nov. 27, 1755. **I** Have just been at Petworth, to view the fissure in the rock near that place, but do not think it remarkable enough to claim the Society's attention. By the best account I can get from

from the people on the spot, it was first discovered by the covering of earth being casually washed away. It was then a small chink, which was afterwards enlarged by children at play; and it is now about two feet wide at the mouth, and two yards high, exactly fronting the south. It is true, that farther in, it shews evident marks of a rent, or violent separation; for the rock on each side is jagg'd into similar breaks; but when this rent was made, is the question. It is certain, that it preceded the late extraordinary agitations of water; all the inhabitants agreeing, that it has continued in the state it is now in near three months. I threw several stones in, and heard them rattle against the sides for a considerable time. A boy was let down to fetch up a dog, that had been thrown in. He went to the length of a cart-rope and an half, which was tied to his body; and, by his account, it appears to be a sort of a concamerated room, though, he says, he did not reach the bottom, but stood upon the edge of a prominent rock. I was in hopes it would have afforded something to confirm the late motions of the water; but, from all circumstances, it seems to be of much longer standing.

As to the ponds near Medhurst, every body agrees, that there was an extraordinary swelling of the water the same day that phænomenon was remarked in yours. The water was thrown several feet above its banks, both at north-mill, at south-pond, and the pond in Lord Montacute's park; and at the first of these, upon its retreat, left some fishes upon dry land. I am, Sir,

Your obliged humble servant,

J. Hodgson.

LETTER



*Extract of LETTER VI.*

*From Cranbrook in Kent, to William Tempest, Esq;  
F. R. S.*

Nov. 7. 1755.

Read Nov. 13, 1755. **T**HE people here are very much alarmed on account of an earthquake, which happened last Saturday, (Nov. the 1st). I felt nothing of it, but some people fancied they did. I do not hear, that the earth moved; only the waters of several ponds, in this and the adjacent parishes, were in such motion, that they overflowed their banks, and then returned back, and overflowed the other side.

*Extract of LETTER VII.*

*Communicated by John Pringle, M. D. F. R. S.*

Chevening, in Kent, Nov. 16, 1755.

Read Nov. 20, 1755. **T**HE accounts from Portsmouth, and other places, about the agitation of the water, I now more readily believe, as the same thing has happened at Tunbridge-town, in the river there, and in a pond at a place, a few miles from hence, called Eaton-bridge; the particulars of which were told to one of our servants by an eye-witness.

The pond is about an acre in size, and across it is a post and rail, which is almost quite covered by the water. Some people heard a noise in the water, and imagining something had tumbled in, ran to see what was the matter; when, to their surprize, they saw the  
water

water open in the middle, so as that they could see the post and rail a good way down, almost to the bottom, and the water dashing up over a bank about two feet high, and perpendicular to the pond. This it did several times, making a great noise. They did not feel the least motion upon the shore, nor was there any wind, but a dead calm. This happened yesterday fortnight, being Saturday the first of November. I suppose we may hear more accounts; and perhaps the same thing happened here, though unobserved.

## L E T T E R   V I I I .

*From Mr. Henry Mills to Thomas Birch, D. D.  
Secret. R. S.*

S I R,

Rotherhithe, Dec. 15, 1755.

Read Jan. 8,  
1756.

**A**S I am informed, that an account of what I observed in the Thames, Nov. 1. would not be unacceptable to you, the fact was briefly this. Being in one of my barges, unloading some timber, between eleven and twelve a clock, both myself and servants were surpris'd by a sudden heaving up of the barge from a swell of the water, not unlike what happens when a ship is launched from any of the builders yards in the neighbourhood. But the state of the tide did not then suit with the launching of ships, and I am since certain, that no ship was launched at that time. After the barge had alternately rose and sunk three or four times with a motion gradually decreasing, the water became quiet again; and I attended no further to an inci-

VOL. 49.

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dent of so short duration, till the reports of the agitation seen in the waters of several ponds brought it again into my memory. Whereupon, consulting my memorandum-book, I found, by the work I was then doing, that the day, on which this happened, was the first of November. I am,

Your most obedient servant,

Henry Mills.

# IX.

*An Account of the Agitation of the Water in Peerless-Pool, near Old-Street, London. By Thomas Birch, D. D. Secret. R. S.*

Read Dec. 18, 1755. **U**PON the reports, which I had received from several gentlemen, that the agitation of the waters observed in many parts of England, Scotland, Ireland, Holland, &c. on Saturday November 1, 1755, had been likewise taken notice of in Peerless-Pool, near Old-street road, being curious to have as authentic and circumstantial an account as possible of a fact, which I had not heard to have been remarked in any other part of London, or its suburbs, I went thither on Saturday December 6, 1755, and took down the following particulars relating to it, from the mouth of one of the two waiters there, who were eye-witnesses of it. He being engaged between the hours of ten and eleven in the morning, with his fellow-waiter, in some business near the wall inclosing the ground, which

which contains the fish-pond, and accidentally casting his eye on the water, was surpris'd to see it greatly moved without the least apparent cause, as the air was intirely ca'm. This occasioned him to call to his companion to take notice of it, who at first neglected it, till being urged to attend to so extraordinary an appearance, he was equally struck with the sight of it. Large waves rolled slowly to and from the bank near them, at the east end, for some time, and at last left the bed of the pond dry for several feet, and in their reflux overflowed the bank ten or twelve feet, as they did the opposite one, which was evident from the wetness of the ground about it. This motion having continued five or six minutes, the two waiters slept to the cold bath near the fish pond, in order to see what pass'd there; but no motion was observed in it by them, or by a gentleman who had been in it, and was then dressing himself, and who, upon being told of the agitation in the fish pond, went directly thither, with the waiters, and was a third witness of it. Upon the ceasing of it, all three of them went to the pleasure bath, between which and the fish pond the cold bath is situated; but they found the said pleasure bath then motionless, but to have been agitated in the same manner with the fish pond, the water having left plain marks of its having overflowed the banks, and risen to the bushes on the sides of them. The waiter likewise inform'd me, that the motion in the fish pond had been observed with great surprize by some persons in a house belonging to Mr. Kemp, the master of Peerless-Pool, situated at a small distance from that pond, and commanding a full view of it.

## L E T T E R X.

*An Account of the Agitation of the Water at Rochford in Essex, Nov. 1, 1755, in a Letter from the Reverend Mr. Thomlinson to the Rev. Dr. Sykes, D. D. Dean of Burion. Communicated by Tho. Birch, D. D. Secret. R. S.*

Christmas-Day, 1756.

Read March 11, 1756. **I**T will surprise you to hear, that we had some symptoms of an earthquake at this place, on the very same day, and about the same hour, in which the late dreadful convulsion of the earth was in so particular a manner fatal to Portugal. One instance of it was of a pond in a close of Mr. Sly's, adjoining to the church-yard; where the water was observed by Mr. Sly himself, to flow a considerable way up the mouth of the pond, and then returning, to flow up the opposite side, repeating this sort of motion for about a quarter of an hour. What is further remarkable in this case is, that, at the very time of this fluctuation, Mr. Sly had the curiosity to examine the condition of two other ponds, which are but a small distance from the former, and found them both very still and quiet. It may deserve notice, that the motion of the water in Mr. Sly's pond was only from east to west, and from west to east, alternately. Whether the fluctuation in those other ponds, which have been mentioned in the news-papers, was the same, it may be worth while: it will at least be a matter of curiosity to inquire. Mr. Sly's pond is very large, and almost round.

round. Its mouth is on the east side. The two neighbouring ponds lie in length from north to south, and are comparatively very narrow in their breadth, from east to west; which may in some measure account for the motion of the water in them, if there was any, being less observable: but I have no reason to suppose there was any, as Mr. Sly could discern none. Mr. Sly, immediately on his return home, informed his family of this very astonishing and unaccountable accident, and had not the least suspicion at that time of there being any earthquake, either here or in any other part of the world.

## L E T T E R   X I.

*From Mr. Richard Philips. Communicated by Lewis Crusius, D. D. F. R. S.*

In Berkshire, near Reading.

S I R,

Strand, Jan. 12, 1756.

Read Jan. 15,  
1756.

**S**INCE I had the pleasure of seeing you, I have received letters from Reading relating to the earthquake, &c. which was felt at two places in that neighbourhood, on the same day, and about the same time, that the great catastrophe happened at Lisbon.

At my sister's request, a gentleman of our acquaintance has been so obliging to go himself to Mr. Pauncefort's, where the effects of it were most remarkable, and had the following account from the person, who felt and saw the whole process of this phenomenon. On the first of November last, at about 11 o'clock in the morning, as Mr. Pauncefort's  
gardener:

gardener was standing by a fish-pond in the garden, he felt a most violent \* *trembling of the earth*, directly under his feet, which lasted upwards of fifty seconds: immediately after which he observed, that the water in the pond was in a very unusual motion, and suddenly thrown on the opposite side, leaving that, on which he stood, quite dry for the space of two yards, and continued in that state for two minutes, or thereabouts, when it returned as before, and collecting in or near the middle of the pond, rose about twenty inches above the level of the water on each side, and continued so for two minutes in violent agitation, which the gardener described to be like the boiling of a pot.

Of the other affair, which happened at Captain Clarke's at Caversham in Oxfordshire, a mile distant from Reading, my sister, who has inquired what the effects of it were there, writes, that Captain Clarke was alarmed with a very great noise, as if part of the house had been falling down: upon examination however it did not appear, that the house was at all damaged; but a vine, which grew against it, was broken off, and two dwarf trees (such as are used in espalier hedges), were split by the shock. She could not learn exactly how long it lasted, but as it happened at the same time as that at Mr. Pauncefort's, it is probable it continued as long as the trembling of the earth did there, which was near a mi-

\* This is the only account, that mentions any *tremor of the earth* to have accompanied the agitation of the waters in this island: and the next account of the very same matter, does not take the least notice of any.

nute. These, Sir, are the particulars of the account I have received, the truth of which may be depended on; and if the communication answers your expectation, I shall be extremely glad of it. I am,

S I R,

Your most humble servant,

Richard Phillips.

## L E T T E R XII.

*An Account of the same. Communicated by the Rev.  
John Blair, LL. D. F. R. S.*

Read Dec. 18,  
1755.

ON Saturday, November 1, 1755, at Earley - Court, near Reading in Berkshire, in a small fish-pond near the house of Edward Pauncefort Esq; the water was observed about eleven a clock in the forenoon to be in a strong agitation, like that of the tide coming in. The first motion of the water was from the south end of the pond to the north end, leaving the ground or bottom of the fish-pond on the south end altogether without water, for the space of six feet. It then returned, and flowed at the south end, so as to rise three feet up the banks, and immediately went back again to the north, where it likewise flowed three feet up the banks; and in the time betwixt the flux and reflux, the water swelled up in the middle of the pond like a ridge, or rising part of land. This motion or agitation of the water, from south to north, and from north to south alternately, backwards and forwards,



forwards, lasted about the space of four minutes of time ; and there seemed to be little or no motion in the direction of east and west, the weather being perfectly calm during the whole time.

### L E T T E R XIII.

*In Oxfordshire, at Shirburn-Castle, the Seat of the Earl of Macclesfield, Pres. R. S. as appears by the following Account, communicated by his Son the Lord Viscount Parker, F. R. S. then upon the Spot.*

Read Nov. 20, 1755. ON Saturday November 1, a little after ten o'clock in the forenoon, walking in the garden at Shirburn-Castle, in Oxfordshire, I perceived the gardener, who was coming towards me by the end of the moat, on a sudden stop short, and look earnestly into the water. As I found by his intentness, that he saw something extraordinary, I went towards him, and perceived immediately a very strange motion in the water. There was a pretty thick fog, not a breath of air, and the surface of the water all over the moat was as smooth as a looking-glass ; notwithstanding which, in that corner of the moat near which I stood, the water flowed into the shore, and retired again successively, in a surprising manner. How long it had done so before I came, or in what manner it began to move, I cannot tell. The flux and reflux, when I saw it, were quite regular. Every flood began gently ; its velocity increased by degrees, till at last, with great impetuosity, it rushed in till it had reached its full height, at which it remained for a little while, and then

then again retired, at first gently ebbing, at last sinking away with such quickness, that it left a considerable quantity of water entangled amongst the pebbles, laid to defend the bank, which run thence in little streams over the shore, now deserted by the water, which at other times always covers it. As the slope of the sides of the moat is very gentle, the space left by the water at its reflux was considerable, though the difference between the highest flood and lowest ebb of these little tides, if I may be allowed the expression, was but about four inches and an half perpendicular height; the whole body of water seeming to be violently thrown against the bank, and then retiring again, while the surface of the whole moat all the time continued quite smooth, without even the least wrinkle of a wave. The time it took up in one flux and reflux, as I did not then observe it, I cannot pretend to guess at. There happened to lie at the bottom of the water, about four feet deep, as I believe, several pieces of white paper; and I could perceive them move backward and forwards, keeping pace with some weeds, and other things, which floated on the top of the water backward and forward, as it ebb'd and flow'd. Being desirous to know, whether the motion was universal over the the moat, I sent a person to the other corner of it, at the same end that I stood, and about 25 yards from me, to examine whether the water moved there or not. He could perceive no motion there, or hardly any; but another, who went to the north-east corner of the moat, diagonally opposite to me, found it as considerable as where I was. As I imagined, that, in all probability, the water at the corner diagonally

opposite to that where I stood, would sink, as that by me rose, I ordered him to signify to me, by calling out, when the water by him began to sink, and when to rise. This he did, but to my great surprise I found, that, immediately after the water began to rise at my end, I heard his voice calling to me, that it began to rise with him; and in the same manner I heard, that it was sinking at his end, soon after I perceived that it began to sink by me. He might stand about ninety or a hundred yards from me. I sent a person to a pond just below where I stood, who called to me in the same manner. The water rose and fell in that pond; but though he stood at the south-west corner of that pond, as I did at the south-west corner of the moat, it did not rise and fall by him in that pond, at the same time as it rose and fell by me in the moat, but sunk sometimes when the moat rose, and rose when the moat sunk, as it seemed by his calling to me, the rising and falling seeming to be quicker than in the moat, though but little: he might stand about forty yards from me. I sent persons to three other ponds, in all which the agitation was very considerable. The swells, that succeeded one another, were not equal, nor did they increase or diminish gradually; for sometimes, after a very great swell, the next two or three would be small, and then again would come a very large one, followed by one or two more as large, and then less again. As I did not know when the water first began to move, I did not think it worth while to observe when it should be quiet again; but having stood by the moat a good while, I went away, and returning again in about half an hour's time, found it perfectly still.

## L E T T E R XIV.

*From John Huxham, M. D. F. R. S. to Mr. William Watson, F. R. S.*

In Devonshire and Cornwall; at Plymouth, Mounts-Bay, Penzance, &c.

Dear Sir,

Read Nov. 27,  
1755.

**F**ROM the 26th of October to the end, the wind was constantly between the north and east, commonly north-east, the barometer 30. 3. thermometer about 50. The north-east wind, and high station of the mercury, continued till the 2d of November; then the wind became west south-west, and the barometer fell somewhat. During all this time the atmosphere was uncommonly thick, and often foggy, but without rain.

Saturday, November 1, about 4 p. m. we had (just about high water) an extraordinary boar, as the sailors call it. The sea seemed disturbed about 20 minutes before, tho' there was very little wind that day, or for some days before. The sky seemed that day very cloudy, in the morning very full of little fiery red clouds, in the afternoon very lowering, and in many places of a very odd copper-colour; the atmosphere excessively thick and dark; insomuch that coming down the northern hill towards Plymouth, one of my servants, who is not very curious, took notice of it to me, and desired I would guard against a storm, as he thought near at hand; but not a drop of rain fell.

B b b 2

When

When I came home, one of our surgeons, who had then just crossed the ferry at Cresson, a mile to the south-east of Plymouth, told me, that the tide had made a very extraordinary out (or recess) almost immediately after high water (about 4. p. m.) left both the passage-boats, with some horses, and several persons, at once quite dry in the mud, though the minute or two before, in four or five feet water: in less than eight minutes the tide returned with the utmost rapidity, and floated both the boats again, so that they had near six feet water. The sea sunk and swelled, though in a much less degree, for near half an hour longer. I was told, that at the next morning's tide there were several very large surges.

This boar drove several ships from their moorings, and broke some of the hawsers, and twirled the ships and vessels round in a very odd manner. At Crunill-passage, over another arm of the sea, about two miles west of Plymouth, the same phenomena were observed; and in Stone-house lake, that communicates with that arm of the sea, the boar came in with such impetuosity, that it drove every thing before it, tearing up the mud, sand, and banks, in a very shocking manner, and broke a large cable, by which the foot passage-boat is drawn from side to side of the lake.

These are the most remarkable appearances we had here, which, I confess, did not much surprise me, as I had formerly noted phenomena of this nature more than once or twice in this port, and even more considerable; an account of one of which you will find in my first Volume of *Epidemics*. But when I heard, that at Portsmouth, Holland, in Ireland,

land, Germany, &c. the waters were so strongly agitated on the very same day, it roused my attention greatly, though I am quite lost in conjecture of the cause. You will please to observe, that it happened not here till about 4 p. m. at Portsmouth about 11 a. m. in Holland about 11 a. m. at Kinsale, &c. in Ireland not till 3 or 4 p. m.

## L E T T E R XV.

*From the Rev. William Borlase, A. M. F. R. S. to the Rev. Charles Lyttleton, LL.D. Dean of Exeter.*

Reverend Sir,

Read Dec. 18.

1755.

**O**N Saturday the first of last month [November] happened in Mount's-bay, and the parts adjacent, the most uncommon and violent agitation of the sea ever remembered. At that time, and for six days before, the weather had been remarkably calm, the barometer higher than I have ever noted it for three years last past, and in all those days Farenheit's thermometer varied only from 51 to 54; which is moderately high with us in Cornwall, where I have not found it to exceed 64 in the hottest months this year, seldom 60.

A little after two o'clock in the afternoon, the weather fair and calm, barometer at the highest, thermometer at 54, the little wind there was being at north-east, about half an hour after ebb, the sea was observed at the Mount-pier to advance suddenly from the eastward. It continued to swell and rise for the space of ten minutes; it then began to retire, running to the west, and south-west, with a rapidity

rapidity equal to that of a mill-stream descending to an undershot-wheel; it ran so for about ten minutes, till the water was six feet lower than when it began to retire. The sea then began to return, and in ten minutes it was at the before-mentioned extraordinary height; in ten minutes more it was sunk as before, and so it continued alternately to rise and fall between five and six feet, in the same space of time. The first and second fluxes and refluxes were not so violent at the Mount-pier as the third and fourth, when the sea was rapid beyond expression, and the alterations continued in their full fury for two hours: they then grew fainter gradually, and the whole commotion ceased about low water, five hours and an half after it began.

Penzance pier lies three miles west of the Mount, and the reflux was first observed here 45 minutes after two: the influx came on from the south-east, and south-south-east: from whence I gather, that the force, from which this agitation proceeded, lay at south nearly, or south-west of this bay, and the sea reaching first the eastern lands (which project a great deal more than those of the west), was thence reflected, and came upon the Mount in an easterly direction: but farther on to the west this eastern current had lost its strength, and the sea came into Penzance from the south-south-east more directly from the point of its momentum. Here the greatest rise was eight feet, and the greatest violence of the agitation about three o'clock.

Newlyn pier lies a mile west of Penzance. Here the flux was observed first, as at the Mount, and came in from the southward (the eastern current being

ing quite spent), nearly at the same time as at the Mount and Penzance, but in a manner somewhat different; it came on like a surge, or high crested wave, with a surprising noise. The first agitations were as violent as any; and after a few advances and retreats at their greatest violence, in the same space of time as at the Mount, the sea grew gradually quiet, after it had rose, to the infinite amazement of the spectators, ten feet perpendicular at least. This is near five feet more than at the Mount pier, and two feet more than at Penzance; and I attribute it to the angle or creek, in which Newlyn lies, wherein the waters were resisted and accumulated by the straitness of the shores, and the bent of the western land; whereas at Penzance the waters were less confined, and consequently could not rise so high; but at the Mount (at that time an island) the sea had full room to spread and disperse itself, and there rose least of all.

The agitations of the sea at Moushole (another pier in this bay) did not materially differ from those at Newlyn.

You may imagine, Sir, that as soon as this extraordinary commotion of the water was observed in a bay so full of people, and in all parts of it so interested (and particularly now the pilchard-fishery is in hand), in what passes upon the sea, all hands were busy in endeavouring to preserve their ships, boats, and craft. At the Mount the fishermen got to their boats, then riding off the pier, concluding that a violent storm was at hand: they were no sooner on board, than their boats were heav'd in with the surge; but they were no sooner in the pier, and struggling.



struggling to secure themselves and boats, as much as their astonishment would permit, than their boats were hurried back again through the gap or mouth of the pier with incredible velocity: when they had gone off as far as the reflux determined, they were carried in, and out again, with an impetuosity, which no ropes could withstand, and would have destroyed both men and boats immediately, if in their passage they had touched the least stone of the pier. As you know the place, Sir, you will easily recollect, that what preserved them was not the rudder, or the oar, but the same stream and current, which put them in danger; for it had neither in or out-let but thro' that narrow gap, and therefore set directly in, and out. At Penzance there was no damage, nor, what is more extraordinary, at Newlyn, where their boats are much more numerous. What preserved them all (under Providence) was, 'that this rage of the water happened a little after noon, when the men were all at leisure to attend their boats: if it had come on upon them in the night, it is thought not one boat out of fifty would have been saved, and consequently many lives lost.

In Guauas lake, the Happy ketch of war veer'd round upon her anchors, keeping her head by turns to the flux and reflux, and in the decline of the commotion hove the log to estimate the velocity of the water, and found it to run at the rate of seven miles in an hour. No motion of the land was any-where perceived, as far as I can learn.

I have no account of this agitation yet from Scilly, but it was perceived at the Land's end; and in the north chanel, at the pier of St. Ives, it rose betwixt  
eight

eight and nine feet, and floated two vessels before quite dry, but all smooth; no sea broke.

In the little harbour of Heyle, about four miles north of the Mount on the Severn sea, the agitation did not make its appearance till an hour and a little more after the ebb began, which must be full an hour later than with us, and is easily accounted for by the circuit of land at the extremity of this county, which the swell must have made before it could reach into the north chanel to St. Ives and Heyle. In this inland half-tide harbour it continued visible but an hour and half; the greatest flux was about the middle of that time, the surge being at that time seven feet high; but in general it rose and fell but two feet only, owing, as I suppose, to the force and quantity of water being broke in its advances into so retired a creek.

At Swansea, in Wales, further up in St. George's chanel, where their ebb is later still than in Heyle, I find the agitation was proportionably later than with us, and was not observed till after two hours ebb, near three quarters after six.

At Kingsale, in Ireland, more indeed to the north of us, but more open to the Atlantic ocean than Swansea, and farther to the west, the agitation reach'd not a full hour after us, but above two hours sooner than at Swansea, all tending to shew, that the force came from the south and south-west.

What relation these little palpitations or tremulous rebounds of the sea (for, in our parts, we may thank God they were no more) had to the dreadful convulsions on the coasts of Spain and Portugal, whether they were the fainter parts of that deplorable

shock at Lisbon, or the expiring efforts of some similar subterraneous strugglings of nature farther to the west, under the Atlantic ocean, will remain uncertain till more facts and dates appear; but, by the accounts from abroad, this first of November seems to have been a day of universal tremor to all the sea-coasts of the western parts of Europe.

I would not be thought to suggest, Sir, that a shock so far off as the coast of Spain could be so immense, as to propagate a motion of the water quite home to our shores. I should rather imagine, that there were several shocks, and some much nearer to us, but all perhaps from one and the same cause diffused in different portions, and permeating more contracted or dilated, but still communicating passages: I should imagine, that this cause affected the seas and land, in proportion to its own force, and the superior or weaker resistance of the incumbent pressure; that where it found the least resistance of all, there it found its vent, and the swell its cure.

But these disputable points I submit with great deference to the learned, who live in the center of intelligence, as well as of mutual information, from one another, and remain,

S I R,

Ludgvan,  
Dec. 4, 1755.

Your most obedient and

affectionate humble servant,

Wm. Borlase.

*Extract*

*Extract of* L E T T E R XVI.

*From Swanzey, dated Decem. 8, 1755. Communicated by the Rev. John Blair, LL.D. F. R. S.*

Read Jan. 8,  
1756.

**O**N the first of November, about two hours ebb of the tide, and between six and seven a clock in the evening, at a place called White-rock, about a mile up this river, where great numbers of ships load coal, and discharge copper-ore, for the use of the works erected at that place, a great head of water rushed up with a great noise, floated two large vessels, the least above two hundred tons (one whereof was almost dry before), broke their moorings, and hove them across the river, and had like to overset them, by throwing them on the banks. The whole did not last ten minutes, the rise and fall; and, what is most remarkable, it was not felt in any other part of the river; so that it must have gushed out of the earth at that place; for near the town and mouth of the river there is a passage-boat, that was passing at that time, and had been for the whole day, it being our market-day, when that boat is constantly employed, and there nothing was felt of it. This is the best account I can get, and what is said to be matter of fact.

*Extract of* L E T T E R XVII.

*From Mr. William Arderon, F. R. S. to Mr. Henry Baker, F. R. S.*

Norwich, January 8, 1756.

Read Jan. 15, 1755. **I**N answer to your inquiry, whether any agitations of the water had been observed in this county on the first of November, give me leave to inform you, that two gentlemen and a servant went out a shooting on the sea-shore, from Sir Thomas L'Estrange's, at Hunston, on the first day of November, and were in great danger of being drowned, by the sea's sudden flowing before its usual time, and saved themselves, with difficulty, by clambering up the sides of the cliffs.

L E T T E R XVIII.

*To Mr. William Arderon, F. R. S. from Mr. Thomas Barber in Yarmouth, concerning the Earthquake on November the first. Communicated by Mr. Arderon.*

Mr. Arderon, January 26, 1756.

Read Feb. 19, 1756. **I** Received yours of the third ultimo, and, after the best inquiry I can make of occurrences here, at the time, that Lisbon was destroyed, can inform you of the following particulars related to me.

John Ives, master of a ship in the London trade, being on board his ship, a little before noon, the first of November (his ship was then in Yarmouth haven,

haven, at the ballast-quay, where the caulkers were repairing her), observed the ship had an uncommon motion, and that the water was violently agitated, and no wind at that time stirring to occasion it. The agitation was so great, that the caulkers left off work some time; and they remarked, that the water suddenly flowed six feet, sloping on the side of the haven.

The uncommon motion of the ships in Yarmouth haven, at that time, by the agitation of the water, was taken notice of by Mr. Gooch, a block-maker, and his man, at work in his shop, fronting the quay; as also by Thomas Aldered, master of a Yarmouth ship, who was then walking on the quay, and by many others.

William Dawson, master of a Yarmouth ship, then going to Gainsborough in Lincolnshire, was inform'd, on her arrival, that the water rose five or six feet, and fell again in a minute or two, both there and at Hull, at that time.

These are the most material circumstances I can inform you of relating to this affair.

## L E T T E R   XIX.

*From the Reverend Mr. John Harrison, Rector of Hawkeshead in Cumberland, to the Right Reverend Dr. Edmund Keene, Lord Bishop of Chester.*

My Lord,

Read Jan. 15,  
1756.

**T**HE last post brought me a letter from Mr. Lambert of Lancaster, intimating your lordship's more particular inquiry into,

into the phenomenon, which happened here the 1st of November last. Some time soon after, I was called to give baptism to a weak child at a house bordering on Windermere-water, about three miles from the head or north end of the lake, and there fell in with four boats, two men to each boat, employed in drawing with nets for chars. I consulted the fishermen, and was told, that but one of their boats was in use the day the waters were disturbed, and that, at the time of such agitation, the boat was drawn up aground, and one of the men on shore, and the other sitting in the boat, the lake quite still and smooth as glass, and not a breath of wind, when on a sudden the water swelled, floated the boat, heaved it up about its length farther upon land, and took it back again in the falling back of the wave; otherwise it would have been left quite dry. I asked, what depth of water the boat takes? and was answered, scarce half a yard. How long the flux and reflux continued? About eight or ten minutes, continually decreasing. What time of the day? About ten in the forenoon, but that, having no watch, they could not be exact. Did the man on land feel no trembling under him, or did they hear any noise? No. I have also inquired of some ferry-men, busy at the time on shore, about the middle part, *i. e.* about five miles from each end of the lake, who gave me the like account in every particular, only that their boat was moor'd, and could not be driven on shore. I asked them, that supposing one had stood at the water's edge while it was still, how deep, they imagined, the swell would have taken him? and was answered, they thought, up to the knees.

Mr.

Mr. Sandys, of Graythwaite, tells me, that his husbandman, and one of his farmers, were that forenoon at work in a field, within sight of the lake, about two miles and a half from the foot or south end of it, and, on their return home, told him, that about ten a clock they heard a noise from towards the water like, as they could imagine, the sound of the slate off the whole side of any large building, sliding down the roof at once; that they expected it to be some strong gust of wind coming at a distance, but as they could not afterwards see a twig stir, they were at a loss to conjecture whence it proceeded; that the water was quite still before and smooth, but on that noise they observed a narrow rippling (to use their own word), from the point of a rock, not far from them, across the lake, but were not high enough to observe any other agitation. I have also consulted a ferryman belonging to Thirston-water, commonly called Conistone-water, a lake about five miles in length. He says he was standing, at the time mentioned, at their landing-place, and was surprised to see the water flow up the bank when there was not the least wind, and the water quite calm. I asked, how high it flow'd? He said, about a yard up the bank, and continued its motion back and forward about five minutes, as near as he could guess, for that he did not look at his watch at the time. How much he computed the perpendicular swell? He thought about a foot; which, as I know the place, I think is pretty near the truth, answerable to a yard-flow up the bank. I am told also, from a person of credit, that the like agitation, though in a less degree, and shorter continuance, was observed



at the time on Easthwaite-water, a lake about a mile and half in length, lying near this town.

In all my inquiry, my Lord, I cannot find, that any motion or tremor of the ground was perceived by any one. And as to the differences of the time the agitations lasted, may they not be thought proportionate to the different dimensions of the lakes, as the vibrations of pendulums, after the impelling powers are taken away, may be found to continue in proportion to their lengths. Upon the whole, tho' the several agitations above-mentioned are undoubtedly matters of fact, yet had there, at the time they happened, been a strong wind, the phenomenon would perhaps have been lost among the common waves; and as no motion or tremor of the ground was perceived, does it not seem hard to account for this motion of the waters from a subterranean cause? Or may it not rather be deemed owing to some sudden alteration in the pressure of the atmosphere? But I shall tire your Lordship, and beg pardon, who am,

My Lord,

Hawkeshead,  
Dec. 24, 1755.

Your Lordship's most dutiful and  
most obedient humble servant,

John Harrison.

*Extract of LETTER XX.*

*From the Honourable and Rev. Dr. Spencer Cowper,  
Dean of Durham, to his Brother the Right Ho-  
nourable William Earl Cowper, F. R. S.*

Durham, near the City.

Read Jan. 15,  
1756.

UPON acquainting Mr. Gowland with Lord Willoughby's desire of being acquainted with the circumstances of the commotion of the water in his pond\*, on the first of November last, he came to me the other evening with his gardener, who gave the following account:

That on the first of November, about half an hour after ten in the forenoon, as he was working in his master's garden, he was alarmed by a sudden rushing noise, like the fall of water: That imagining the head of the pond to be broken down, which was at a small distance from him, he ran hastily towards it, and finding it firm and intire, was a good deal at a loss to account for the noise he had heard; when, casting his eye upon the water, he saw it gradually rise up, without any fluctuating motion, till it reached a grate, which stood some inches higher than the common water-level, through which it discharged itself for a few seconds: That it then subsided as much below the mark it rose from, as it was above it in its greatest elevation; and continued thus rising and falling all the time he stood by it, which was, he reckons, about six or seven minutes, making four or five returns in about one. At last

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\* At Mr. Gowland's seat, five miles from Durham.

he grew so alarmed, that he ran to the house to call his fellow-servants down to look at it; but though the water then continued to have some commotion, it was nothing considerable. Mr. Gowland very much regrets not being made acquainted with it at the same time; but he having company in his house that day, the servants did not mention this extraordinary appearance to him till the day following, when he went down to the pond, but found all quiet. The gardener did not observe the least shaking of the ground; but in regard to the noise, he said, it was much greater than could proceed from the running of the water through so small a grate, nor did he observe it at any of the discharges he saw the water make through it. The pond stands to the west from the house, and is about forty yards long, and ten broad. The ebb and flow were each about half a foot in the perpendicular. This is all the account the man gave; which, though very extraordinary, agrees so nearly with most others given of this commotion in different parts of Europe, that the truth of it need not be doubted, particularly as Mr. Gowland was apprised of this appearance the day following, many days before the others, which confirm it, was known in England.

A remarkable high tide was observed near Hartlepool, on the fifth of November. At a place called Saltholm it rose upwards of a yard higher than ever known in the greatest spring-tides, and did some damage. The time was, as well as I can recollect, eleven at night. I cannot hear, that the earthquake was felt at all in this county.

## L E T T E R   XXI.

To Dr. John Stevenson, *Physician at Edinburgh.*  
*Communicated by Dr. John Pringle, F. R. S.*

Scotland, at Loch Ness.

Dear Sir,

Read Jan. 8, 1756. **I** Have your favour of the 20th, and in compliance with your request, I give you the following reply to your queries.

I arrived at Fort-Augustus, from Fort-William, on the 31st of October last : next morning about ten I walk'd abroad, when the barrack-master, and several others, came and acquainted me, that they had seen a very extraordinary agitation of the waters at Loch Ness. I refused giving credit to their story, and a little afterward returned to the fort.

About 11, my clerk and the brewer at that place came and acquainted me, that a more extraordinary agitation than the former had happened, and they apprehended some danger to our brewery, which is situated where the river Oich discharges itself into the lake. I walked then to that place, but before my arrival the water had returned to its usual channel. I saw very clearly the marks on the banks, to which it had flowed ; the banks were quite wet, and a strip of leaves of trees and twigs, &c. left on them. I inquired then into this affair, and the account the spectators gave were, that they observed the river Oich, which runs from west into the head of the lake, swell very much, and flow up the river from the lake, with a pretty high wave, about two or three feet higher than the ordinary sur-

face, with a pretty quick motion against the wind, and a rapid stream, about two hundred yards up the river; then broke on a shallow, and flowed about three or four feet on the banks on the north side of the river, and returned again gently to the lake. That it continued ebbing and flowing in that manner for about an hour, without any waves so remarkable as the first, till about 11 o'clock, when a wave higher than any of the rest came up the river, and, to the great surprize of all the spectators, broke with so much force on the low ground, on the north side of the river, as to run upon the grafs upwards of thirty feet from the river's bank.

Lieut. Smith, of the artillery, Mr. Gwyn, son of Captain Gwyn, of the Loch Ness galley, Mr. Lumisden, barrack-master at Fort-Augustus, Mr. Forbes, barrack-master of Bernera, Thomas Robertson, brewer at Fort-Augustus, and George Bayne, my clerk, and several others, were the spectators of this extraordinary phenomenon: some of them saw the whole progress of it, others only a part.

Loch Ness is about twenty miles in length, and from one to one and a half mile broad; bears from south-west to north-east. There was no extraordinary muddiness observed in the water, though it did not appear quite so clear as usual. The morning was cold and gloomy, and a pretty brisk gale of wind blowed from west south-west. The river Oich lies on the north side of the fort, and on the south side runs the river Tarff from west south-west, discharging itself also into the head of the lake; and which was observed to be agitated at the same time and manner as the other. But there were no shakings

or tremors felt upon the land. I have heard of the same agitations in Loch Lommond. It is probable all the other lakes in the highlands were also agitated, and it is my opinion they were, though I have not heard of many particulars. I always am, with greatest regard,

My dear Doctor,

Edinburgh,  
Dec. 22, 1745.

Your very affectionate

humble servant,

Robert Gardener.

*P. S.* Loch Ness is vastly deep, its soundings in many places being from 100 to 135 fathom, which is greatly below the level of the sea at Inverness. Its sides are most part rocky, and it deepens immediately from them. About three musket-shot from the river Oich it measures about 120 fathom in depth.

*Extract of LETTER XXII.*

*From Sir James Colquhoun, of Lufs, to Mr. Fletcher, Member of Parliament, dated Rosedoe, Dec. 8, 1755. Communicated by Dr. Pringle, F. R. S.*

At Loch Lommond.

S I R,

Read Jan. 22,  
1756.

**Y**OUR favour of the 29 ult. I received by last post, desiring me to transmit you an exact account of our Loch Lommond earthquake. You must know, that I was confined to the house with a sore throat, which probably

bly prevented my seeing that strange phenomenon: but the best account I have got of it is from Angus M'Diarmid, inn-keeper at Tarbat, who was an eye-witness, and was with his watch in his hand all the time of the agitation; and his account is as follows :

On the first day of November last, Loch Lomond all of a sudden, and without the least gust of wind, rose against its banks with great rapidity, but immediately retired, and in five minutes time subsided, till it was as low in appearance as any body then present had ever seen it in the time of the greatest summer drought; and then it instantly returned toward the shore, and in five minutes time rose again as high as it was before. The agitation continued at this rate from half an hour past nine in the forenoon till fifteen minutes after ten, taking five minutes to rise, and as many to subside; and from fifteen minutes after ten, till eleven, every rise came somewhat short in height of the one immediately preceding, taking five minutes to flow, and five to ebb, until the water settled as it was before the agitation, Angus M'Diarmid measured the height, to which the loch rose, and found it to be two feet four inches perpendicular. Loch Long and Loch Ketterin were also agitated on the same day, and about the same time; but the phenomenon was not so minutely observed as that any exact account can be got of it.

It appears, by communications sent from abroad, that the like agitations of the water were observed at the Hague, Leyden, Harlem, Amsterdam, Utrecht, Gouda, and Rotterdam, and also at Bois-le-Duc; about eleven of the clock on the 1st of November; and likewise

at

at Kingfale in Ireland, as appears from the following letter.

# L E T T E R XXIII.

*From L. Nicola to James Simon, Esq; F. R. S.*

S I R,

Read Jan. 22,  
1756.

**A**S I have been desired to write an account of the extraordinary agitations of the sea here, on the fatal first of November, which may possibly be acceptable to the Royal Society, I have taken the liberty of directing it to you as a member of that Society, to transmit or suppress, as you may think proper.

On the first of November, between the hours of two and three afternoon, the weather being very calm, and tide near full, a large body of water suddenly poured into this harbour, with such rapidity that it broke the cables of two sloops, each moor'd with two anchors, and of several boats lying between Scilly and the town, which were carried up, then down, the harbour, with a velocity far exceeding what I ever saw by a ship or boat, though favoured with all the advantageous circumstances of tide and wind, in any degree of violence. By the direction, in which the sloops were carried down, I imagined they and several boats would have ran foul of each other, and also of the King's (or revenue) boat, which lay at her moorings: but just at the time I thought this unavoidable, an eddy whirled them round several times, and hurried them up again with the same rapidity. This was repeated several times;



times; and while the current rushed up at one side of the harbour, it poured down with equal violence at the other. A vessel, that lay all this time in the pool, did not seem to be any ways affected; nor was the violence of the currents much perceived in the deeper parts of the harbour, but raged with most violence on the flats. The bottom of the harbour, which is all a slab, was much altered, the mud being washed from some places, and deposited in others. The Reverend Mr. Keef says, the perpendicular rise of the water at his quay was five feet and a half, as he measured it, and I am told it was much higher at the market-quay, which it overflowed, and poured into the market-place, as a gentleman who was present assures me; and also says, that it came with such rapidity, that some men, who were on the quay immediately, on the first rise of the water, ran off, but could not do it with expedition enough to prevent their being overtaken, and up to the knees. The agitations of the water were communicated some miles up the river, but, as in the harbour, were mostly perceivable in the shallowest places. The successive risings and fallings of the water continued about ten minutes, as nearly as I could judge, not having particularly attended to this circumstance, and then the tide returned to its natural course. Between six and seven in the evening I saw the water rise again, tho' not with as great violence as the former time; and I am told it continued its alternate ebbs and flows till three in the morning. By different accounts received here the water was affected in the same manner along the coast, to the westward of this harbour, and it is reported, that, about nine o'clock

o'clock in the morning a shock of an earthquake was felt at Cork; but I have not met any persons of credit who pretend to say they felt it.

I am informed that, in the year 1750, something of this nature happened here. The water suddenly flowed out of the river, which is of a very considerable depth near a mile up (See PLATE XI.), with such rapidity as to leave its chanel almost dry, and raise the water in the harbour so suddenly, that several boats were drove from their anchors, and cast on shore. This, attended with violent thunder and lightning, continued scarcely a minute, when the water returned to its natural situation.

These are all the particulars I was a witness to, or could collect from good authority. I am,

S I R,

Kingfale,  
Dec. 7, 1755.

Your most humble

and obedient servant,

L. Nicola.

## L E T T E R XXIV.

*Communicated by Richard Brocklesby, M. D. F. R. S.*

S I R,

Read Nov. 27, 1755. **M**Y letter from Kingfale runs thus: " There was a violent shock of  
" an earthquake felt yesterday at Cork, and at the  
VOL. 49. E e e " fame

‘ same moment of time we had a most surprising  
 “ thing happened here: it was then ebbing, and  
 “ near low water, when on a sudden the flood re-  
 “ turned so violently, and in such quantities, that  
 “ the market-quay was covered, and the tide flowed  
 “ quite up to the market, at the same time not a  
 “ breath of wind stirring; all the vessels in the har-  
 “ bour of every kind were forced from their anchors,  
 “ and hurried away by the rapidity of the flood; and  
 “ as the ebb was as sudden, and equally violent, se-  
 “ veral of the vessels were left dry on shore.”

*N. B.* The water must have rose considerably above  
 its utmost height at even equinoctial tides, to flow  
 up to the market. This letter was dated Nov. 2.  
 and wrote by a person of great veracity. I am,

S I R,

Nov. 19, 1755.

Your most obedient servant,

Hen. Tom.

## L E T T E R XXV.

*An Account of an extraordinary Alteration in the Baths of Toplitz in Bohemia, on the first of November 1755, in a Letter from Father Joseph Steplin to James Short, M.A. F. R. S. Translated from the Latin.*

S I R,

Prague, January 30, 1756.

Read Feb. 26,  
1756.

**A** Report being brought hither, that at Toplitz, a village famous for its baths, and nine Bohemian miles north-west from Prague, the source of these baths had undergone some change; in order to know the truth of this, I requested the president of the Supreme Royal Council to send me an exact account of it, in answer to the several questions, which I proposed to him. By this means I have procured the following relation of that fact: That in the year 762 those baths were discovered; from which time the principal spring of them had constantly thrown out the hot waters in the same quantity, and of the same quality. On the first of November of the last year 1755, between eleven and twelve in the morning, the chief spring cast up forth such a quantity of water, that in the space of half an hour all the baths ran over. About half an hour before this vast increase of the water the spring grew turbid, and flowed muddy; and, having stopped intirely near a minute, broke forth again with prodigious violence, driving before it a considerable quantity of a reddish oker, *Crocus Martialis*. After which it became clear, and flow'd as

E e e 2

pure

pure as before; and continues still to do so; but it supplies more water than usual, and that hotter, and more impregnated with its medicinal quality.

You see, Sir, that the alteration made in this spring happened almost at the very time when Portugal began to be shook with the earthquake. Not doubting therefore, that accounts from various parts concerning the effects of so extensive an earthquake will be sent to the Royal Society, I presum'd, that this would not prove unacceptable to them. I am,

S I R,

Your most humble servant,

J. Steplin.

*Extract of* L E T T E R XXVI.

*From Mr. De Hondt, of the Hague, concerning the Agitation of the Waters, Novem. 1. 1755. Translated from the French, and communicated by Tho. Birch, D. D. Secret. R. S.*

Hague, Nov. 7, 1755.

Read Nov. 20, 1755. **W**E had, on Saturday morning last, at eleven o'clock, a phenomenon, which astonish'd every body. In absolutely calm weather there was observed of a sudden so violent a motion in the water, that the ships were struck against each other, and broke the cables, which fasten'd them. It was felt at the same time at the Hague, Leyden, Harlem, Amsterdam, Gouda, Utrecht, Rotterdam, and Boissleduc. At the Hague it was but.

but slight; and no motion was felt in the ground. You will see a more circumstantial account of it in the Dutch Gazette of this week.

*Extraët of L E T T E R XXVII.*

*From Monsr. Allamond, Professor of Philosophy at Leyden, and F. R. S. dated there Decem. 9, 1755, to Mr. Trembley, F. R. S. Translated from the French.*

Read Dec. 18,  
1755.

**A**LL that I have been able to collect, that is certain, with relation to what was observed here concerning the earthquake of the first of November, amounts to but very little. Between half an hour after ten and eleven in the morning, in some of the canals of this city, the water rose suddenly on the quay, situated on the south. It returned afterwards to its bed, and made several very sensible undulations, so that the boats were strongly agitated. No-body could inform me to what height the waters rose, except a miller, who lives between Delft and the Hague. Having observed this singular motion of the water in a canal, he ran home for a pole, which he plunged into the water, and saw it rise up the pole at least a foot high, though the air was extremely calm.

The same kind of motion was perceived here in the tuns of water of two brewhouses, and in those of three brewhouses at Harlem. The branches of the Roman catholic church of Rotterdam, which hung from long iron rods, made several oscillations. A tallow-chandler at the Hague was surprised to  
hear

hear the clashing noise made by all the candles hung up in his shop.

This is all that I have any certain information of, and it shews, that our country had some share in the general agitation. Many other things are reported, but not sufficiently attested.

The accounts brought from Norway inform us, that the same observations were made there, almost at the same time.

LIX. *An Account of the Earthquake, Novem. 1, 1755, as felt in the Lead Mines in Derbyshire; in a Letter from the Reverend Mr. Bullock to Lewis Crusius, D. D. F. R. S.*

L E T T E R I.

Rev. Sir, Ashford, near Bakewell, Derbyshire, Mar. 3, 1756.

Read March 11,  
1756.

I Here send you an account of the earthquake, which happened at the lead mines on Eyam-edge in the peak of Derbyshire, on Saturday the 1st of Novem. 1755, about 11 o'clock in the forenoon. I made a strict inquiry at the mines, on the 21st of February last, both for my own and your satisfaction; and can assure you, that the circumstances related may safely be relied upon as matter of fact. If there be any thing in my power, which you would chuse to have explained

more particularly, please to communicate, and it shall be done with the greatest pleasure by,

S I R,

Your most obedient and

obliged humble servant,

Wm. Bullock.

Francis Mason, overseer, says, That he sat in a little room, which he uses to write in: it stands about forty yards from the mouth of one of the engine shafts. He felt one shock, which very sensibly raised him up in his chair, and caused several pieces of lime or plaister to drop from the sides of the room: the roof of it was so violently shook, that he imagined nothing less than the engine shaft was run in; whereupon he immediately went out to see, and, contrary to his expectation, found the shaft open, and all things about the spot in their proper order.

Upon inquiry, I was informed by the aforesaid Francis Mason, that in a field about 300 yards from the mines, there had happened a chasm or cleft on the surface of the earth, which was supposed to be made at the same time he felt the shock, for the following reason: It lies close by a road, which he uses daily to and from the mines: in the morning when he came, there was nothing uncommon to be seen, but on his return at evening he observed a cleft about one foot deep, and six inches in diameter; its continuation from one end to the other, was near 150 yards,



yards, being parallel to the range of the vein on the north side. When I examined it, which was upwards of three months after the shock happened, the length of it was not much more than 60 yards, though I could perceive some vestiges of its farther continuance: the depth of it was about eight or nine inches, and its diameter four. As the soil was very light, and the season has been remarkably wet, it is highly probable, that the fissure is considerably closed since it was first made. These were the most remarkable circumstances, which happened upon the surface of the earth. Though my inquiry was of every one in particular, that was there employed about the mine, the concurrence of whose testimonies might seem more strongly to confirm the account; yet I look upon it as unnecessary to trouble you with every man's story, which would be only a repetition of, or something similar to, what has been before related.

William Hallom, and Jo. Howson, miners, say, That at the aforesaid time they were employed in carting, or drawing along the drifts the ore and other minerals to be raised up the shafts. The drift, wherein they were working, is about 60 fathoms, or 120 yards deep, and the space of it from one end to the other 50 yards, or upwards. Hallom was at the end of the drift, had just loaded his cart, and was drawing it along, but was suddenly surprised by a shock, which so terrified him, that he immediately quitted his employment, and ran to the west end of the drift to his partner, who was not less terrified than himself. They durst not attempt to climb the shaft, lest that should be running in upon them, but con-

consulted what means to take for their safety. Whilst they were thinking of some place of refuge, they were alarmed by a shock much more violent than the former; which put them in such a consternation, that they both ran precipitately to the other end of the drift. There was a miner working at the forfield, or east end of the vein, about six fathoms below their level, who called out to them, imagining they were in danger of being killed by the shafts running in upon them, which he supposed was the case; and told them, if by any means they could get down the shaft to him, they would be more secure, because the cavity, where he was working, was encompassed with solid rock. They went down the shaft to him, where, after observing they had neither of them received any misfortune, he told them, that the violence of the second shock was so great, that it caused the rocks to grind one upon another. His narration was interrupted by a third shock; and, after an interval of about four or five minutes, was succeeded by a fourth; and about the same space of time after, by a fifth; none of which were so violent as the second. They heard after every shock a loud rumbling in the bowels of the earth, which continued for about half a minute, gradually decreasing, or appearing at a greater distance. They imagined, that the whole space of time, from the first shock to the last, was about twenty minutes; and they tarried about ten minutes in the mine after the last shock; when they thought it adviseable to examine the passages, and to get out of the mine, if possible. As they went along the drifts, they observed, that several pieces of minerals were dropped from

the sides and roof, but all the shafts remained intire, without the least discomposure.

The space of ground at the aforesaid mines, wherein it was felt, was 960 yards, which was all that was at that time in workmanship.

## L E T T E R. II.

*An Account of the Earthquake at Lisbon \*, Nov. 1. 1755, in Two Letters from Mr. Wollfall, Surgeon, to James Parsons, M. D. F. R. S.*

Dear Sir,

Lisbon, Nov. 18, 1755.

Read Dec. 18,  
1755.

**A**S some years ago I had the pleasure of occasionally meeting you at Dr. Hill's house, I venture to take the liberty of sending you an account of the terrible earthquake, which has so lately destroyed this city. If you have any

\* This city suffered greatly by an earthquake in 1531, thus described by Paulus Jovius. Hist. L. 29. fol. 180. Nec multo post sequenti mense Januario, par prope exitii clades Lusitanis in ipsis proculdubio visceribus terræ furente debacchanteque Atolo illata est, cujus impetu urbs Ulyssipo in primis incomparabile detrimentum sensit, & post eam coloniam, Sanctæarenæ & Azumberga, Almerinique urbium publica privataque ædificia, inusitato terræ motu conquassata proscissaque magnam vim mortalium ruinis oppressere, & nonnulla etiam navigia turbulento inflati maris hiatu absorpta sunt, sicut Tagus infans allidentis maris fluctibus repulsus, discedentibus in utranque ripam suis undis sicca in medio vada oblitupentibus cunctis ostenderet, nemoque jam tota prope Lusitania tectis suis confideret, subsultante scilicet solo, ideoque non cito destitutam terræ motus vim ita præmonstrante, ut regis & reginæ exemplum sequuti universi fere incolæ tabernacula apertis in locis castrensi in more erigere cogerentur; neque in his tamen soluti metu pernoctarent, quum repentinum aliquem terræ hiatum, quo absorberi possent, non temere extimescendum arbitrantur.

other correspondents in this place, I doubt not but they are able to give you a more satisfactory relation of it than I will pretend to: but, if you have not, such an account, as the present hurry of my spirits will permit me to send you, will doubtless be more agreeable to you, than the uncertain reports, that you will find in the news-papers. All that I can pretend to at present is to communicate to you “a round unvarnish’d tale”; and that I will do with all the truth and candour imaginable.

It perhaps may be necessary previously to tell you, that since the beginning of the year 1750, we have had much less rain than has ever been known in the memory of man, excepting the last spring, which gave such a supply of rain, as has produced very plentiful crops: the summer has been cooler than usual, and for the last forty days, fine clear weather, without being remarkably so. On the first instant, about 40 minutes past nine in the morning, was felt a most violent shock of an earthquake: it seemed to last about the tenth part of a minute, and then came down every church and convent in town, together with the King’s palace, the magnificent opera-house, joining to it; in short, there was not a large building in town that escaped. Of the dwelling-houses there might be about one-fourth of them that tumbled, which, at a very moderate computation, occasioned the loss of thirty thousand lives. The shocking sight of the dead bodies, together with the shrieks and cries of those, who were half buried in the ruins, are only known to those who were eye-witnesses. It far exceeds all description, for the fear and consternation was so great, that the most resolute person durst not stay a moment to remove a few stones

off the friend he loved most, though many might have been saved by so doing: but nothing was thought of but self-preservation; getting into open places, and into the middle of streets, was the most probable security. Such, as were in the upper stories of houses, were in general more fortunate than those, that attempted to escape by the doors; for they were buried under the ruins with the greatest part of the foot-passengers: such as were in equipages escaped best, though their cattle and drivers suffered severely; but those lost in houses and the streets are very unequal in number to those, that were buried in the ruins of churches; for as it was a day of great devotion, and the time of celebrating mass, all the churches in the city were vastly crowded, and the number of churches here exceeds that of both London and Westminster; and as the steeples are built high, they mostly fell with the roof of the church, and the stones are so large, that few escaped.

Had the misery ended here, it might in some degree admitted of redress; for though lives could not be restored, yet the immense riches, that were in the ruins, might in some part have been digged out: but the hopes of this are almost gone, for in about two hours after the shock, fires broke out in three different parts of the city, occasioned from the goods and the kitchen-fires being all jumbled together. About this time also the wind, from being perfectly calm, sprung up a fresh gale, which made the fire rage with such fury, that at the end of three days, all the city was reduced to cinders. Indeed every element seemed to conspire to our destruction; for soon after the shock, which was near high water, the  
tide

tide rose forty feet higher in an instant than was ever known, and as suddenly subsided. Had it not so done, the whole city must have been laid under water. As soon as we had time for recollection, nothing but death was present to our imaginations.

For 1st, the apprehensions of a pestilence from the number of dead bodies, and the general confusion, and want of people to bury them, were very alarming: but the fire consumed them, and prevented that evil.

2d. The fears of a famine were very great; for Lisbon is the store-house for corn to all the country, for fifty miles round: however, some of the corn-houses were happily saved, and though the three succeeding days to the earthquake an ounce of bread was worth a pound of gold, yet afterwards bread became moderately plenty, and we were all happily relieved from our starving condition.

The 3d great dread was, that the low villainous part of the people would take an advantage of the confusion, and murder and plunder those few, who had saved any thing. This in some degree happened; upon which the King gave orders for gallows immediately to be placed all round the city; and after about a hundred executions, amongst which were some English sailors, the evil stopp'd. We are still in a state of the greatest uncertainty and confusion, for we have had in all twenty-two different shocks since the first, but none so violent as to bring any houses down in the out-skirts of the town, that escaped the first shock; but no body yet ventures to lie in houses; and though we are in general exposed to the open sky, for want of materials to make tents, and tho'

rain.

rain has fallen several nights past, yet I don't find but the most delicate tender people suffer their difficulties with as little inconvenience as the most robust and healthy. Every thing is yet with us in the greatest confusion imaginable: we have neither cloaths nor conveniencies, nor money to send for them to other countries. All Europe is deeply concerned in the immense riches and merchandizes that are lost, but none so much as our own nation, who have lost every thing they had here. Few English lives have been lost in comparison of other nations, but great numbers wounded; and what adds to the misfortune is, that we are three English surgeons, but neither instruments, bandages, nor dressings, to relieve them. Two days after the first shock, orders were given to dig for the bodies, and a great many have been taken up, and recovered. It is amazing some instances of recoveries, that I could send; in short it is amazing, that we are not all lost. I lodged in a house, where there were thirty-eight inhabitants, and only four saved. In the city prison 800 were lost, 1200 in the general hospital, a great number of convents of 400 in each lost; the Spanish ambassador with 35 servants. It would be too tedious to enter into particulars, for I procured this paper by mere accident, and I write this on a garden wall. If you are pleased to communicate the contents to your Society, I beg you will first please to dress it up in different language. It fortunately happened, that the King and the Royal Family were at Belime, a palace about a league out of town. The palace in town tumbled the first shock, but the natives insist, that the Inquisition was the first building

ing that fell down. The shock has been felt all over the kingdom, but along the sea-side more particularly. Faro, St. Ubals, and some of the large trading towns are, if possible, in worse situation than here; though the city of Porto has quite escaped.

It is possible, that the cause of all these misfortunes came from under the western ocean, for I have just been in conversation with a captain of a ship, who seems a very sensible man, who tells me, that he was fifty leagues off at sea; that the shock was there so violent as greatly to injure the deck of his ship; it occasioned him to think, that he had mistaken his reckoning, and struck upon a rock, and they instantly hawled out their long-boat to save themselves, but happily brought the ship, though much injured, into this harbour.

If there are any further particulars, Sir, in which I can gratify your curiosity, I will most gladly endeavour to do it. And I hope my future correspondence will be more clear and correct than this confused letter I send at present. If in any thing I can give you pleasure, please to address to me, Surgeon, at Lisbon. I am

Your most obedient and

most humble servant,

Richard Wolfall.



L E T T E R III.

Dear Sir,

Lisbon, Nov. 22, 1755.

Read Dec. 18,  
1755.

A Few days ago, when 'I did myself the pleasure to communicate to you an account of the terrible earthquake, that has destroyed this city, I omitted a very material circumstance; which was to acquaint you of the time that it lasted, which was between five and seven minutes. The very first shock was extremely short, but then it was as quick as lightning, succeeded by two others, which, in the general way of speaking, are mentioned all together as only one shock. About twelve o'clock we had a second shock. I was then in the Terra do Paço, or King's palace-yard, and then I had an opportunity of seeing the walls of several houses, that were standing, open from top to bottom, more than a quarter of a yard, yet close again so exactly as to leave no signs of injury. I am,

S I R,

With great esteem,

Your most obedient humble servant,

Richard Wolfall.

*P. S.* Since my last we have had some very heavy rains, and had only one shock, and that not violent, the last four days.

L E T T E R

## L E T T E R IV.

*A Copy of Part of Two Letters, written by John Mendes Saccheti, M. D. F. R. S. to Dr. DeCastro, F. R. S. dated from the Fields of Lisbon, on the 7th of November, and the 1st of December, 1755.*

Read Jan. 15,  
1756.

I Cannot, at present, give you a proper account of the first day of this month of November, the most unhappy and fatal day for this court and kingdom, the cause of our total ruin, and general loss of so many nations, connected with the flourishing condition of the commerce of this great capital ; because I am, and have begun, to write the natural, philosophical, and meteorological history of this tragical event, with a design to publish it, by the particular desire of his excellency the secretary of state Diego de Mendoca Corte Real, and some other noblemen, which, as soon as finished, I shall take special care to send it to you, and communicate it to our Royal Society. Therefore now I shall only inform you, in a few words, that *cecidit Babylon*, and that there is no more to be seen than *Campus ubi Troya fuit*. Lisbon, the great city of Lisbon, was at once tumbled to the ground, burnt to ashes, and plundered ; reducing this court, and the most part of the realm, to inconceivable misery, and deplorable condition. The third part of its inhabitants was buried in the ruins, and the other two thirds wandering, scattered, and full of misery in their tents. I have miraculously escaped with a friend and family, where I lodged, and all retired to his

country-house, where we do not know yet, as every body else, friends of their friends, fathers of their children, husbands of their wives; because every one fled away from their habitations, full of terror, confusion, and distraction.

This year has been with us very rainy and wet; the three preceding ones excessively dry, insomuch that some springs, formerly very plentiful of water, were dried, and totally lost; at the same time the predominant winds were east and north-east, accompanied with various, though very small, motions or tremblings of the earth, and, in the year 1750, we had a very sensible one.

The day before the fatal earthquake the atmosphere, and light of the sun, had the appearance of clouds and notable offuscation, and more strong and visible at the actual time of the great shock, which was by undulation, and lasted from six to eight minutes. The weather was rather warmer than commonly we have it at this time of the year, and had continued so for several days before. In all this time were predominant the east and north-east winds.

It ruined not only this populous city, but all the southern part of the county of Estremadura, and a great one of the kingdom of Algarve.

I am almost positive, that the earthquake was progressive, and that this place was the center of it. Its direction, according to my own observations, from north-west to south-west.

The earth opened in fissures in several parts, but neither fire or visible smoke came out of it.

The water in the sea rose several times, and in a few minutes made three fluxes and refluxes, rising above

above the greatest spring-tides two fawns, or fifteen English feet. This phenomenon happened three days before the new moon, and the earthquake, when the high tide had run up three parts of it.

*Extract of LETTER V.*

*From Mr. J. Latham, dated at Zfufqueira, Dec. 11, 1755, to his Uncle in London. Communicated by Peter Daval, Esq; Secret. R. S.*

S I R,

Read Jan. 15, 1756. **P**ERhaps you may have heard of the dismal calamity, that befel the city of Lisbon, by an earthquake and fire, which laid the whole city in ruin. I was on the river on Saturday the 1st of November, with one of my customers, going to a village three miles off. In a quarter of an hour the boat made a noise as if on the shore or landing, though then in the middle of the water. I asked my companion, if he knew what was the matter? He stared at me; and, looking at Lisbon, we saw the houses falling, which made him say, *God bless us, it is an earthquake.* About four or five minutes after, the boat made a noise as before, which was another shake. We saw the houses tumble down on both sides of the river. In Lisbon, a convent on a high hill fronting the river, the most part of it came down, a great many were killed, and buried in the ruins; many tumbled neck and heels in the water, others ran down to the river, up to their middle and necks. A strong northerly wind blew from shore, which covered the water with dust, and in

our boat we could scarce see one another; and it intirely hid the sun from us for some time, though as fine a morning as ever was, and as warm a sun as is with you in the month of July. The wind soon dispersed the dust, the shaking seemed over. In about three quarters of an hour we came to the village, where we were called ashore, and met several gentlemen, who came out of the city on horse-back, but so frightened, that they did not know what it was: we told them what we had seen, and in a quarter of an hour after our landing, the village was alarmed with another shake. We got down to our boat, in order to get in; in a moment's time the river rose so high, as obliged us to take to our heels, and run for our lives, into the fields and high ground, the water flowing across the road, which, from the low tide, was above a quarter of a mile: the ships were whirl'd about, and several people taken into the water, others driven ashore, and dashed to pieces. From the high grounds we could see the sea at about a mile's distance come rushing in like a torrent, tho' against wind and tide. A fine new stone quay in Lisbon, where the merchants land their goods, where at that time about three thousand people were got out for safety, was turned bottom upwards, and every one lost; nor did so much as a single body appear afterwards. It being a holy-day, great numbers of the natives being at their devotion in convents and churches, whose large buildings suffered most, it is computed about sixty thousand souls, and a hundred and odd of the foreigners, and all sorts of cattle, perished. The religious houses being illuminated with wax-lights; and the images dress'd, by the shakes

were

were set on fire by night, in several places, and by Monday morning intirely consumed, with the rich furniture of convents, nunneries, and nobility's houses, and all the merchants and tradesmen's goods, besides jewels, gold, plate, and coined money. There have been a great many shakes by nights and days : even on the 8th of December was felt a strong one ; it was much more violent in some places than others. The ground was opened ; in some places you might put your hand down broad-ways, and not feel the bottom with a long stick. A sea port, called St. Ubal's, was intirely swallowed up, people and all. It is thought Lisbon would have shared the same fate, had it fronted the sea. The greatest factory belonging to Great Britain was ruined in 24 hours ; the merchants, who used to vie with our principal gentry in house-keeping, equipage, and dress, are so reduced, that ten or twelve live in a small house, others seek for lodgings, or make what shift they can, while a great many are gone to their own countries.

## L E T T E R VI.

*Observations, made at Colares \*, on the Earthquake at Lisbon, of the 1st of November 1755, by Mr. Stoqueler, Consul of Hamburg. Communicated by Mr. Henry Baker, F. R. S.*

Read Feb. 5, 1756. **T**HE 31st of October the weather was clear, and uncommonly warm for the season ; the wind north, from which quarter,

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\* It is about twenty miles from Lisbon, and lies behind the rock, about two miles from the sea

about four o'clock in the afternoon, there arose a fog, which came from the sea, and covered the valleys; a thing very common in the summer, but rare in this season of the year. Soon after the wind changing to the east, the fog returned to the sea, collecting itself, and becoming the thickest I ever saw. As the fog retired, the sea rose with a prodigious roaring.

The 1st of November, the day broke with a serene sky, the wind continuing at east; but about nine o'clock the sun began to grow dim, and about half an hour after we began to hear a rumbling noise, like that of carriages, which increased to such a degree as to equal the noise of the loudest cannon; and immediately we felt the first shock, which was succeeded by a second and third; on which, as on the fourth, I saw several light flames of fire issuing from the sides of the mountains, resembling that which may be observed on the kindling of charcoal.

In the spot, on which I remained till the third shock was over, I observed the walls to move from east to west. Removing from thence to another situation, from whence I could discover the sea coast, I observed from one of the hills called the Fojo, near the beach of Adraga, that there issued a great quantity of smoke, very thick, but not very black; which still increased with the fourth shock, and after continued to issue in a greater or less degree. Just as we heard the subterraneous rumblings, we observed it would burst forth at the Fojo; for the quantity of smoke was always proportioned to the subterraneous noise. This I saw continue till the noon of the 2d of November, when I retired from the

the place where I had observed it. It continued to smoke some days longer, more or less, according to the subterraneous rumblings.

When I returned to Colares a fortnight after, I saw no smoke; neither was there any sign of it, even when the noise was to be heard; but I could meet with no-body, who could inform me when the smoke had ceased.

The 20th in the afternoon, being on the former spot, I saw a small fog coming from the sea (from the same quarter whence the smoke appeared), which smelt of sulphur; and the wind returning to the east, the fog retired to the sea; and in the morning of the 21st, about nine o'clock, we felt two shocks of an earthquake sufficiently violent, but no more smoke was seen. I cannot say, whether the same phenomena preceded the earthquake of the 11th, because I was not at the place, nor had I any-body there to make the observation.

I went to examine the place, from whence I saw the smoke arise, but I did not discover from whence it could have issued; nor did I find any signs of fire near the place: from whence I infer, either that the smoke exhaled from some eruption or volcano in the sea, which the waters soon covered, or that, if it issued from some chasm in the land, it closed afterwards. I rather incline to the former opinion, because it is natural, that the water should retire from the place of the eruption: besides, the sea having risen in some places, it is probable, that it fell in others; and indeed it has visibly retired there, for you may walk on the dry shore now, where-before you could not wade. And the second conjecture  
may



may be true, as some chafms on the dry land are now almost closed up, and others intirely so.

*Particular Observations.*

In the afternoon of the 31st of October, I observed, that the water of a fountain was greatly decreased: on the morning of the 1st of November it ran very muddy, and after the earthquake it returned to its usual state, both in quantity and clearness. Some fountains, after the earthquake, ran muddy, some decreased, others increased, others were dried up; and one, that with the earthquake was dried up intirely, returned two days after to its usual state. In some places where there was no water, springs burst forth, which continued to run. On the spot of Varge, and river of Macaas, at the time of the earthquake, many springs of water burst forth, and some spouted to the height of 25 palms \*, throwing up sand of various colours, which remained on the ground. On the hills, numbers of rocks were split, and there were several rents in the ground, but none considerable. On the coast, pieces of rock fell, some of them very large, and in the sea sundry rocks were broken: the most noted are those called by the sailors Sarithoes, or Biturecras, of which one was only broken off at the summit, the other all to pieces.

N. B. Between these rocks and the main, the coasting vessels failed at low water; and now you may

\* The Portuguese palm is about nine inches.

go to them at low water, without wetting of your feet. From the rock called Podra de Alvidrar (situated where the smoke issued), a kind of parapet was broke off, which issued from its foundation in the sea. In a swamp or lake, which received a good deal of water in winter, and was not dry in summer, the earth rose; for there is now scarcely the appearance of a hollow, which was before to the depth of six or seven palms; it now remains even with the adjacent ground. In other places, by the change of the currents it appears, that the earth was moved, so that some spots are more elevated, others more depressed than before.

In the afternoon of the 24th I was much apprehensive, that the following days we should have another great earthquake (from which it pleased God to preserve us), for I observed the same prognostics as in the afternoon of the 31st of October; that is, the weather being pretty serene, the wind northerly, the fog came from the sea towards the vallies, and the wind changing to the east, the fog retired to the sea; however not so thick as that of the 31st of October. The sea was soon in great agitation, and roaring. And I further observed, that the water of a fountain began to be disturbed to such a degree, that in the night it ran of a yellow clay colour; and from midnight to the morning of the 25th, I felt five shocks, one of which seemed to me as violent as that of the 11th of December.

I was informed, that there was some bituminous matter, but could find none. Indeed I once picked

up a stone split through the middle, whose edges seemed to me to have sulphur lodged on them; but I was then in a hurry, and never could find the place where I had taken it up.

*Extract of LETTER VIII.*

*To Tilman Henkel, Esq; Merchant in London, concerning the Earthquake at Oporto in Portugal, Nov. 1, 1756. Communicated by Mr. John Ellicot, F. R. S.*

Read Nov. 27, 1755. **T**HIS goes by Captain Richard Knowler, and serves chiefly to acquaint you, that last Saturday, the 1st instant, we had such a terrible earthquake here, that all of us were afraid of being swallowed up alive, tho', thank God, it did but very little damage; and now, I hope, it is quite over. It began about half an hour past nine o'clock in the morning, just like thunder, or rather the rattling of a coach over stones; and my own house, as well as most other people's, during the first shock, which was a very terrible one indeed, was just as if it was in a convulsion, which lasted, without exaggeration, at least seven or eight minutes, and every thing shook and rattled in it all the while, exactly as if it was coming down, which frightened people so much, that a great many ran, with all their family, into the streets, where I plainly saw the earth heave, with people walking upon it, just as if it was in labour, and as it really was, the like never known in this country before. At six o'clock  
at

at night there was another great shock; but that was soon over, and there were several inferior ones before, and the next day likewise; but all last night, thank God, every thing was very quiet. What frightened people likewise very much, was the river, which rose and fell surprisngly every quarter of an hour, for upwards of four hours at least, four or five feet, and sometimes more; and some saw the river in some places open, and throw out a vast deal of wind, which was very terrifying, as nobody could tell what would be the end of it. In short, for my part, I must confess, I never was so much frightened in my life; and the description I have given you of it, I can assure you, is not near so shocking as the thing really was itself; and I pray God we may never have such another.

## L E T T E R IX.

*Extract of Two Letters to Mr. Plummer, Merchant in London, from Oporto, concerning the Earthquake felt there. Communicated by Mr. John Ellicot, F. R. S.*

Novem. 1, 1755.

Read Nov. 27, 1755. **T**HIS morning, betwixt nine and ten o'clock, this city was alarmed with the terrible shock of an earthquake, which lasted violently the space of five or six minutes, but has done no further damage than the overturning some pedestals from the tops of some churches, and cleaving the walls of some old houses. While it lasted, the city was all in consternation, and the shock

was perceived in the river, among the shipping, by a sudden flux and reflux of the tide, but no damage. Pray God defend us, on our guard, in all his judgments, and avert them from us.

The earthquake, violent as it was, I don't hear has done any very great damage: four or five boats were overset by it on the bar, but no one drowned; some vessels were drove from the moorings, but received no hurt: several large stones were thrown from the tops of buildings, but without touching anybody; but some, through mere fear, have died: amongst these is N. Webber, who was let blood about five, and went off in the night: others too received hurt by crouding out of churches, which were much thronged, it being a mass-day. Had it happened in the night, instead of nine in the morning, short as the duration was, I fear the consequence would have been very fatal; for even as it was, several people were for throwing themselves from their windows, to get into the streets, which they would undoubtedly have done, if in the night; and in the hurry, even down stairs, many would have broke their limbs, and perhaps lost their lives. So strongly has the shock affected the minds of some, that they imagine six or seven more have been felt since; but, for my own part, I know of one only, and trust in God I shall never experience the like, for it was really very terrible.

I forgot to observe to you, that, during the time of the earthquake, and indeed preceding it, was heard a hollow dreadful noise, but I did not observe any disagreeable smell, or alteration in the air, the sky being as serene as usual, and the after-part of the day without a breath of air.

*Extract*

*Extract of LETTER X.**From Oporto, dated Novem. 1, 1755.*

Read Nov. 27, <sup>1755.</sup> **W**E have been greatly alarmed to-day by an earthquake; the shock was so great that really it was terrible: we all ran into the street, the churches were also deserted, and every body seemed frightened out of their senses, as if they thought the world was at an end! It began about ten o'clock in the morning, and continued ten minutes; but, thank God, I do not hear of any considerable damages done; which, considering the violent manner in which the houses were shaken, and the continuance of it, is very providential. God grant it may have a proper effect on the minds of all. It was equally felt on the river, and, I am told, was rather more in Villa Nova, and in Gaya: and two Brazil ships, that had just got over the bar, were all at once forced into the harbour again, and very narrowly escaped being lost.

*Extract of LETTER XI.*

*From Oporto, by the Western, Captain Knowler, dated Novem. 1, 1755. Communicated by Theodore Jacobson, Esq; F. R. S.*

Read Nov. 27, <sup>1755.</sup> **I** Was never so alarmed in my life, as this morning, between nine and ten o'clock, when we were all at breakfast: the house began

began to tremble in such a manner that we all started. I ran down-stairs, and, when I came into the street, I saw all the world running out of their houses, and crying out, An earthquake. I made for the water, and in my way I thought the last day was come, by the the cries of the people, and the hurly-burly about the church door, where many had like to be lost, endeavouring to get out: it lasted for about six minutes, and in a quarter of an hour after, we had a second, and in about an hour a third. These two last were nothing equal to the first, and the water rose and fell about four feet every minute for a considerable time, but now runs its natural course, and we begin to be ourselves again. Several chimnies fell, and some towers, and a great many walls cracked in the city, but at a league distance several houses fell, and we fear we shall hear of a good deal more damage from the country. This moment we have had another shock, and it is now six in the evening.

*Extract of* L E T T E R XII.

*From Mons. Muyfflon to Mons. Bazin, concerning the Earthquake at Oporto, Nov. 1, 1755. Communicated by M. Maty, M. D. F. R. S. Translated from the French.*

Oporto, Nov. 2, 1755.

Read Nov. 27, 1755. **Y**esterday at nine in the morning we felt here a terrible earthquake, which continued near six minutes, and, as you may imagine, threw us all into a great consternation. Two other shocks were felt afterwards, but less vio-

lent than the first. The river swell'd considerably, and two ships, which were at the bar, going out, were driven back into the port.

*Extract of LETTER XIII.*

*From Madrid, to the Spanish Consul, residing in London. Translated from the Spanish. Communicated by Mr. John Ellicot, F. R. S.*

Madrid, Nov. 3, 1755.

Read Nov. 27, 1755. **T**HE only motive of my writing is to give you an account of the terror and astonishment, with which we were seized at Madrid. On the day of All Saints, in the morning about ten o'clock, and some few minutes more (five by two of my watches), there was very sensibly felt a great earthquake: according to the common opinion, it lasted five or six minutes. Every body at first thought, that they were seized with a swimming in their heads; and, afterwards, that the houses, in which they were, were falling. The same happened in the churches, so that the people trod one another under foot in getting out; and those, who observed it in the towers, were very much frightened, thinking, that they were tumbling to the ground. It was not felt by those, who were in their coaches, and very little by those, who walked on foot. No remarkable accident happened, excepting that two lads were killed by the falling of some pieces of a cross, which was over the door of a church belonging to a monastery. Every body is much frightened, and there are a great many indisposed  
by



by the fright. It was felt in most of the houses, without receiving any remarkable damage, excepting the churches of St. Philip, St. Thomas, Portaceli, and towers of St. Trinity, and St. Millan, which will require to be examined by skilful workmen. In the bishop's chapel, in the parish of St Andrew, and in the new palace, although it was much felt, it did not the least damage. It is said, that it was felt more in the Escorial; and, to increase their fright, a fire broke out at the same time, in the kitchen belonging to the body guards. Their Majesties immediately gave orders to come to Madrid. They arrived, accompanied by only one or two coaches, at Retiro, about eight at night, where they put up tents for that night, and in the morning went to Atocha, to a tent, where they staid till noon. We are very anxious to know how far it extended itself.

#### L E T T E R XIV.

*An Account of the Earthquake at Cadiz, Novem. 1, 1755, in a Letter from Mr. Benjamin Bewick, Merchant there, to Mr. Joseph Paice, Merchant in London.*

Cadiz, Nov. 4, 1755.

Read Dec. 18, 1755. **I** Shall remember this week a long time, for that and something of another nature, I am going to relate, as you may not have so faithful a relation from the news-papers.

The 1st instant, just before ten, the whole town was shook with a violent earthquake, which lasted, as far as I can gather from the curious; above three  
minutes

minutes and an half. To give you an idea of its violence, the water in the cisterns (which are underground) washed backwards and forwards so as to make a great froth upon it. Every body ran out of the houses and churches, in a terrible consternation, but no damage was done, as all the buildings here are excessively strong. We thought ourselves very safe when this was past, and people recovered a little the fright. An hour after, looking out to sea, we saw a wave coming at eight miles off, which was at least sixty feet higher than common. Every body began to tremble; the centinels left their posts, and well they did. It came against the west part of the town, which is very rocky: the rocks abated a great deal of its force. At last it came upon the walls, and beat in the breast-work, and carried pieces of eight or ten tun weight, forty and fifty yards from the wall, and carried away the sand and walls, but left the houses standing, so that only two or three persons were drown'd. Every one now thought the town would be swallowed up; for although this was run off, yet with glasses we saw more coming. The people were in the utmost consternation, and ran some one way, some another. The governor ordered the gates to be shut, that people might not go out of town, as the land was lower than the town; by which means, he saved the lives of thousands, who wanted to fly, they did not know where. When the wave was gone, some parts, that are deep at low water, were quite dry, for the water retired with the same violence it came with. These waves came in this manner four or five times, but with less force each time; and about one the sea grew more calm, but

was still in a boiling motion. Every thing was washed off the mole. The bay was full of barrels, and boats, and timber; but no damage was done to the shipping. The walls have suffered very much. There is a neck of land, that goes from thence to the island of Leon, open to the ocean on one side, and to the bay on the other, but very narrow: upon this was a very strong causey, which the sea has washed away, as if it was nothing, that in some places you cannot say, here is the road. It has almost opened a communication between the sea and the bay, which will be a great misfortune to the town, as it will spoil the bay, now become a small island. It is to be hoped the governor will endeavour to prevent that. There were about forty or fifty people drowned on the causey, and a great many beasts. Some of the towns about us have suffered a great deal more than we, by the falling of houses and towers, but we have not yet got an account of what damage has been done; and it is believed, by the course of the earthquake, that it did not go to Gibraltar. The whole day the weather was extremely serene and pleasant. Since the sea is come to itself, and nature seems to be recovered from her convulsions. We had rain, and a fresh wind, so that it is hoped we shall have no more of these dreadful calamities, which are shocking to human nature. God grant it may be so. These phænomena are very uncommon here. About 25 years ago there was a small shock of an earthquake, but not attended with these horrid risings of the sea. Should they be frequent, there would be no living here; the town would even be destroyed, and we  
have

have no place to fly to, unless it is aboard the ships, which are at a distance. Thank God it is no worse. Adieu : believe me ever yours.

*N. B.* The day of the earthquake the weather was as clear and serene as the finest summer-day in England.

Benjamin Bewicke.

## L E T T E R    X V .

*An Account of the Earthquake at Cadiz, in a Letter to the Spanish Ambassador at the Hague, from Don Antonio d'Ulloa, F. R. S.*

Read Dec. 18, 1755. ON the first of this month [November] we had here an earthquake, the violence of which was not inferior to that, which swallowed up Lima and Callao, in Peru, towards the end of October 1746. It happened in very fine weather, at three minutes after nine in the morning, and continued five minutes, and consequently near twice as long as that of Peru, the duration of which was only three minutes. If every thing was not destroyed here, it seems particularly owing to the solidity of the buildings. The inhabitants had scarce begun to recover from their first terror, when they saw themselves plunged into new alarms. At ten minutes after eleven they saw rolling towards the city a tide of the sea, which passed over the parapet of sixty feet above the ordinary level of the water. At thirty minutes after eleven came a second tide ; and these two were followed by four others of

the same kind, at eleven o'clock fifty minutes; twelve o'clock thirty minutes; one o'clock ten minutes; and one o'clock fifty minutes. The tides continued, with some intervals, till the evening, but lessening. They have ruin'd 100 toises of the rampart; part of which of three toises length, and of their intire thickness, were carried by the torrent above fifty paces.

The ships were exposed to eminent danger; the greatest part of them were driven afloat; but most of them fortunately were saved, some by veering their cables, others by securing themselves by new anchors; so that only one Swedish ship and some boats were lost. A great number of persons perished on the causey, which leads to the isle of Lesu. Seville has been greatly damaged. St. Lucar and Cheres have likewise suffered much; and Conel is said to be intirely destroyed.

## L E T T E R XVI.

*An Account of the Earthquakes, that happened in Barbary, inclosed in a Letter from General Fowke, Governor of Gibraltar, to the Right Honourable Henry Fox, Esq; one of his Majesty's principal Secretaries of State: Communicated by the Right Honourable Philip Lord Viscount Royston, F. R. S.*

### *Tetuan.*

Read March 4, 1756. **T**HE earthquake began the 1st of November, at ten in the morning, and lasted between seven and eight minutes; during which space the shock was repeated three different

different times, with such violence, that it was feared the whole city would fall down: but the only damage, that resulted, was the opening or parting of some of the walls of fundry houses. It was likewise observed, that the waters of the river Chico, on the other side of the city, and those of a fountain, appeared very red.

*Tangier.*

The earthquake began about the same time, but lasted longer than at Tetuan; the trembling of the houses, mosques, &c. was great, and a large promontory of an old building near the city gate, after three shocks, fell down to the ground, by which five shops were demolished: the sea came up to the very walls, a thing never seen before, and went down directly with the same rapidity as it came up, as far as the place where the large vessels anchor in the bay, leaving upon the mole a great quantity of sand and fish. These commotions of the sea were repeated eighteen times, and continued till six in the evening, though not with such violence as at the first time. The fountains were dried up, so that there was no water to be had till night: and as to the shore side, the waters came up half a league inland.

*Arzila.*

It happened about the same time, but the damage was not great. At the coming up of the sea seven Moors, who were out of the town walls, were drowned; and the waters came in through one of the city gates very far. The water came up with  
such

such an impetuosity, that it lifted up a vessel in the bay, which, at the water's falling down to its center again, fell down with such a force upon the land, that it was broke to pieces; and a boat was found at the distance of two musket-shots within land from the sea.

*Salle.*

There happened a very great damage, several houses having fallen down. The waters came up with such a rapidity, that they came into the city, and at their falling down, great quantities of fish were found in the streets, and many persons were drowned: two ferry-boats overset in the river, and all the people on board were also drowned; and a large number of camels, that were just then going for Morocco, were carried away by the waters.

*Fez.*

Vast numbers of houses fell down, and a great many people were buried under the ruins. At the Scloges (a place where the Barbarians live, not far from Fez), a mountain broke open, and a stream issued out as red as blood.

*Mequinez.*

Vast number of houses fell down, and a great many people of both sexes were buried under their ruins; the convent of the Franciscan friers fell down to the ground, but the friers were saved.

*Saffe.*

Several houses fell down, and the sea came up as far as the great mosque, which is within the city, and at great distance from the sea.

*Morocco.*

*Morocco.*

By the falling down of a great number of houses many people lost their lives ; and, about eight leagues from this city, the earth opened, and swallowed up a village, with all the inhabitants (who were known by the name of the sons of Bufunba), to the number of about 8 to 10,000 persons, together with their cattle of all sorts, as camels, horses, horned cattle, &c. and soon after the earth was closed again, in the same manner as it was before.

*Fez and Mequinez.*

On the 18th of November there happened another earthquake, which was more violent than the first, and lasted till break of day the 19th ; during which time great numbers of houses fell down at Fez ; many people of both sexes were buried under their ruins ; and as to Mequinez, there are but few houses left standing. The people killed by the falling of the houses, besides the wounded, are numberless ; and in the part of the town called the Jews habitation, only eight persons were saved.

*Sarjon Hills.*

One of the said hills was rent in two ; one side of which fell upon a large town, where there was the famous sanctuary of their prophet, known by the name of Mulay Teris ; and the other side of the said hill fell down upon another large town, and both towns and the inhabitants were all buried under the said hill.



*Taffo.*

This famous city was wholly swallowed up; no remains left.

This last earthquake was likewise felt at Tetuan and Tangier, but without any other damage than that the fountains of Tangier were dried up for the space of twenty-four hours.

These are the truest and freshest advices, that have been received at Gibraltar, to the 1st of January 1756.

## L E T T E R XVII.

*An Account of the Earthquake in the Island of Madeira, Nov. 1, 1755, in a Letter from Dr. Tho. Heberden, to his Brother Dr. William Heberden, F. R. S.*

Read Jan. 8,  
1756.

**N**OVEMBER the 1st, 1755, in the city of Funchal, on the island of Madeira, at half an hour past nine o'clock in the morning, was perceived a shock of an earthquake. The first notice thereof was a rumbling noise in the air, like that of empty carriages passing hastily over a stone pavement: immediately the floor moved with a tremulous motion, vibrating very quickly; the windows rattled, and the whole house seemed to shake. The shock lasted a full minute; during which the vibrations, tho' continual, abated and increased twice very sensibly, in point of force (not unlike what I have observed in an echo from the discharge of a fowling-piece, opposite to a range of mountains, whence

whence the sound has reverberated with reciprocal intensions and remissions). The increase, after the first remission of the shock, was the most intense; the door of the room I was in vibrating to and fro very remarkably then, which it had not done before; neither did it afterwards in the second increase.

The noise in the air, which had preceded the shock, continued to accompany it; and lasted some seconds after the motion of the earth had entirely ceased; dying away like a peal of distant thunder rolling through the air. The direction of the shock seemed to be from east to west.

About an hour and half after the shock had ceased, the sea, which was quite calm (it being a fine day, and no wind stirring) was observed to retire suddenly some paces, and, arising with a great swell, without the least noise, as suddenly advancing, overflowed the shore, and entered into the city. It arose full fifteen feet perpendicular above high water mark, although the tide, which ebbs and flows here seven feet, was then at half ebb. The water immediately receded again, and, after having fluctuated four or five times between high water and low water mark, the undulations continually decreasing (not unlike the vibrations of a pendulum) it subsided, and the sea remained calm as before this phenomenon had appeared.

The season of the year has been more than ordinarily dry; the rains, which generally begin to fall the beginning of October, not having set in as yet (Nov. 10). The weather for some weeks preceding the earthquake has been very fine and clear, but the day previous thereto (October 31), was very remark-

ably fair and serene, as was the former part of the day on which it happened : but the afternoon was very dull and dark, the sky being intirely overcast with heavy black clouds : the subsequent day was very fair.

The greatest height of the thermometer the three last days in October, and on the first of November, (the day of the earthquake) was — 69°  
 November the second it rose to — 71  
 The barometer had been stationary several } 29 28  
 days, at — — — }  
 November the second it rose to — 30 1

In the northern part of this island the inundation has been more violent, the sea there retiring at first above a hundred paces, and suddenly returning, overflowed the shore, destroying or damaging several houses and cottages, forcing open doors, and breaking down the walls of several stores or magazines, and carrying away in its recess a considerable quantity of grain, &c. Above two hundred pipes of wine are computed to be lost on this occasion ; great quantities of fish were left on the shore, and in the streets of the village of Machico. All this has been the effect of one sole undulation of the sea, it never flowing afterward so high as high water mark ; although it continued fluctuating much longer there, before it subsided, than here at Funchal, as the fluctuation and swell was much greater here than it had been farther to the westward, where in some places it has been hardly, if at all, perceptible.

## L E T T E R XVIII.

*Another Account of the same Earthquake at Madeira, in a Letter from Mr. Charles Chambers to his Father, in London, dated at Madeira, Novem. 1, 1755. Communicated by Lewis Crusius, D. D. F. R. S.*

Nov. 1, 1755. 3<sup>8</sup> minutes past 9 in the morning.  
 Read Jan. 8, 1756. **T**HIS moment happened an earthquake, which lasted more than a minute (I believe near two): though of so long a duration, I do not think it so terrible as that in 1748, possibly owing to the present being in the day-time; but, I assure you, it shook every thing about me very effectually.

*P. S.* Three quarters past eleven; there is a circumstance attends, or rather follows, the earthquake, which is very surprizing, and, I confess; shocks me more than the thing itself; which is, the sea all on a sudden rises as though it were the main body of it, and in many parts runs quite to the wall, and at the prattick house, in at the gate. I say, as though it were the main body, as there is no more swell at other times, than in the midst of summer, with a light easterly breeze, and such as has blown all day. I was just called on to observe this, and thought proper to mention it. God, of his infinite mercy, preserve us from all disasters.

November 3. Thank God we have had no return of the earthquake, nor was the rising of the sea at-

K k k 2 tended

tended with any bad consequences in our part of the island; though we hear, that at Mechico and Porto da Cruz it did considerable damage: from the latter of which places it carried off near 200 pipes of wine, and several caldeoras [stills for distilling brandy], and when the sea recoiled, it was so far from its usual limits, as to discover the foundation of rocks, that never before had been seen, and which lay as far distant from the shore as our Ilheo\*.

*N. B.* The port, where all the business is done, is called Funchal, and lies in the south part of the island. Mechico is a bay, with a village four leagues to the eastward. Porto da Cruz is the north-eastermost part of the island, about six leagues distant from Funchal. The distance referred to from the shore to the Ilheo, something more than 200 yards.

## L E T T E R XIX.

*An Account of the late Earthquakes of Novem. 1, and 9, 1755, as felt at Neufchâtel in Swisserland, in a Letter from Mons. De Vautravers, F. R. S. to Thomas Birch, D. D. Secr. R. S.*

Neufchâtel, Decem. 13, 1755.

Read Jan. 22, 1756. **T**HE dreadful earthquake of the 1<sup>st</sup> of November last has been perceived even in this country, though very faintly. It turned some of our rivers on a sudden muddy, with-

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\* A rock whereon a castle is built.

out any rain, and swelled our lake of Neufchâtel to the height of near two feet above its natural level, for the space of a few hours. Some time before nine, three weeks after the earthquake, we had continual rains and storms, generally from north-west, the air very mild, which is not usual at this season amongst us.

The 9th of this month we felt a much more severe shock of an earthquake. It happened a little before three o'clock in the afternoon, with a vibratory motion from west to east; another from east to west, and a third from west to east again. The stove in our servants room was thereby split in several places. Some chimnies fell in at Cudrefin; the bell in the tower at Morat rung two strokes; but no other misfortune has happened, which I have hitherto heard of. The shock was severer in lofty places than it was in low grounds. The lake of Morat, immediately after the earthquake, sunk three inches, and remains still in the same depression. The same earthquake was felt the same day, at the same hour, at Basil, Berne, Fribourg, Geneva, and all over Switzerland; as likewise at Besançon in France. Whether it extended itself any farther is not yet known.

This kind of phænomenon being unusual in this country, and its dangerous effects having been newly heard of from Portugal and Spain, all our inhabitants were much alarmed, and apprehensive of some other great misfortune in other countries.

Immediately after the earthquake, from frosty weather, we had ever since a damp and mild air, great fogs, and to-day plenty of snow. It has been observed, that these three years successively the inhabitants

bitants of Swisserland have suffered great droughts in the summer-seasons, so as to dry up several springs and rivulets, which were never known in any man's memory to have been dried up.

*Extract of LETTER XX.*

*An Account of the Earthquake felt at Geneva, Novem. 9, 1755, in a Letter from Monsr. Trembley, Professor of Mathematics there, to his Brother Mr. Abraham Trembley, F. R. S. Translated from the French.*

Geneva, Decem. 16, 1755.

Read Jan. 8, 1756. **I** Cannot tell whether the earthquake of November 1, was felt here. It was felt at Lyons. It is said, that the waters retired for some moments at the end of the lake of Geneva; and that a motion was observed in those of the lake of Zurich.

On the 9th of this month, a little before half an hour after two in the afternoon, in very fine and very calm weather, there was felt here in all the houses in general a very great shock of an earthquake; but it did no damage. The motion was particularly remarked in looking-glasses and windows. Those, who were sitting, perceived, that their chairs shook; and many thought, that they were going to fall. The sick felt the motion in their beds. The bells in the rooms of several houses rang. The bell of the clock in the tower of the isle of Rhone rung several times. The motion was felt even on the ground floor of houses. I was then walking upon the  
Treille,

Treille \*, and felt nothing. People are not absolutely agreed concerning the direction of the motion. The greatest part of them represent it to have been from north to south, as it was in the preceding earthquake felt here in March 1753, about two in the afternoon, in very fine weather likewise; but it was not near so generally felt as this last. This of the 9th of December was felt at Nion, Morges, Lausanne, Berne, Zurich, and perhaps more strongly than here.

An acquaintance of mine, who was then in the country, at half a league distance from this city, saw his garden-wall make three vibrations, and he was seized himself with a swimming in the head.

Three shocks were in fact felt within the space of about a minute. During the first a noise was heard like that of a cart passing over a pavement.

## L E T T E R   XXI.

*An Account of the Earthquake felt at Boston in New-England, Novem. 18, 1755. Communicated by John Hyde, Esq; F. R. S.*

Boston, Novem. 24, 1755.

Read March 11, 1756. **N**OVEMBER 18, 1755, being Tuesday, about half an hour past four in the morning, I was awaked by the shaking of my bed, and of the house; the cause whereof I immediately concluded could be nothing but an earthquake, having experienced one before. The trem-

\* A pretty high terrace.



bling (for as yet it was scarce more) increasing, I soon got out of bed, and went towards the window on the other side of the chamber, to observe, if there was any thing unusual in the appearance of the sky, or heavens. By the time I had got about half way across the room, which might be six or seven seconds from my first awaking, the shaking was a little abated; so that I imagined the height of the shock was past. But this thought no sooner came into my mind, than I found how much I was mistaken; for instantaneously the shock came on with redoubled violence, and loud noise: the windows, doors, chairs, &c. being prodigiously agitated; and indeed the whole house rocking and cracking to such a degree, that I concluded it must soon fall, or be racked to pieces, unless perhaps it should be swallowed up intire. Having first just looked out at the window, I hastened down stairs, unbolted and opened the door, with an intention to go into the street, thinking, though without reason, almost every place freer from danger than that where I was: but, upon opening the door, I found the shock was something abated, and having looked out at the door a moment or two, returned to my chamber, and opened a window, at which I stood for the space of five or six seconds; the shaking and the noise were by this time much lessened, and still kept decreasing, as though all would very soon become still and quiet. However, there was after this a little repetition both of the trembling and the noise, though no ways to be compared to what had been before. I then went to the other side of the chamber for my watch, returning with it to the window, in order to observe the time, which I did seven or eight seconds

seconds before the shock was intirely over, it being then thirty-one minutes after four. People, I perceive, differ very widely respecting the whole duration of the earthquake, from the first apparent symptoms of it, till it was intirely over, some supposing it to have been six or seven minutes, some four or five, and others scarce more than one. According to the best computation I am able to make, which is from what I did during the continuance of it, removing from one place to another, as related above, I think it could be but little more, and certainly not much less, than two minutes. There was another shock about an hour and ten minutes afterwards, which, though small in comparison to the former, was yet universally perceived by those who were awake. The visible effects of the earthquake are very considerable in the town; to be sure much more considerable than those of any other earthquake, which has been known in it. Many chimnies, I conjecture (from observation) not much less than an hundred, are levelled with the roofs of the houses: many more, I imagine not fewer than 12 or 1500 are shattered, and thrown down in part; so that in some places, especially on the low loose ground, made by encroachments on the harbour, the streets are almost covered with the bricks that have fallen. Some chimnies, though not thrown down, are dislocated, or broken several feet from the top, and partly turn'd round, as upon a swivel; some are shov'd on one side horizontally, jutting over, and just nodding to their fall: the gable ends of several brick buildings, perhaps of twelve or fifteen, are thrown down, and the roofs of some houses are

quite broken in by the fall of the chimnies : some pumps suddenly dried up ; the convulsions of the earth having choaked the springs that supplied them, or altered their course. Many clocks were also stopped by being so violently agitated.

These are the most considerable effects of the earthquake, which have fallen under my observation ; for the shaking of pewter, &c. from the shelves seems hardly worth mentioning after them.

It is said earthquakes are usually preceded and followed by a great noise, which several say they heard with this ; but myself did not perceive any noise in this instance, which I could take to be distinct from the concussion and rattling of the things upon the surface of the earth, added to the roaring of the sea, which roaring had been something greater than is usual for several days before.

Some persons likewise speak of observing a glimmering light at the beginning of the shock, which lasted for some time. But I have no remembrance of this, though I observed with care, and now endeavour to recollect whatever was remarkable respecting a phenomenon so unusual in this part of the world, and so justly terrible in all.

## L E T T E R XXI.

*An Account of the Earthquake felt in New York, Novem. 18, 1755, in a Letter from Cadwallader Colden, Esq; to Mr. Peter Collinson, F. R. S.*

Decem. 9, 1755.

Read Mar. 18,  
1756.

**O**N the 18th of last November, a few minutes past four in the morning, I was awaked with the shock of an earthquake, I felt the bed under me, and the house, shaking, in such a manner as to alarm me greatly. I plainly heard the noise like that of carts on pavements, going to the eastward, with now and then a noise like the explosion of a great gun at a distance. It was felt about four a clock at Philadelphia, and half after four at Boston, and was more violent to the eastward than the westward; and there was an eruption at a place called Scituate, about twenty or thirty miles to the southward of Boston.

We have had the driest summer and autumn that ever was known: for some days before the earthquake, though the sky was perfectly calm and serene, the air was so light, that the smoke of the town by falling down was offensive to our eyes, as we walked the streets; and my watch, for some time before it, went unusually slow.

In the last remarkable earthquake, which happened about seventeen years ago, and nearly at the same time of year, the weather preceding it was much the same as now, attended with the falling of the smoke in the town.

## L E T T E R XXII.

*An Account of the Earthquake felt in Pennsylvania, Novem. 18, 1755, in a Letter to Mr. Peter Collinson, F. R. S. dated at Philadelphia, Decem. 1. 1755.*

Read Mar. 18, 1756. **A**BOUT four of the clock the 18th of November last, this province was pretty generally alarmed with the shock of an earthquake, which occasioned great consternation, but I do not hear of any damage that ensued, unless the breaking some China-ware and glasses. I heard no noise like that of a rushing wind, which was heard in many places; but felt a gentle shaking of my bed, in such a manner as convinced me what it was, which continued to increase, and the windows by the increasing trepidation of the earth began to rattle; some China on a chest of drawers were moved pretty much. As I conjectured, it gradually increased for one minute, to such a degree as to open my chamber door, by drawing the bolt of the lock out of the staple.

Some people think they felt its continuance five or six minutes, but I think it did not exceed two, nor was it less. I felt the shock of the two earthquakes in England; but they were little in comparison to this.

*E N D of P A R T I.*





I. A. R. I. 75.

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